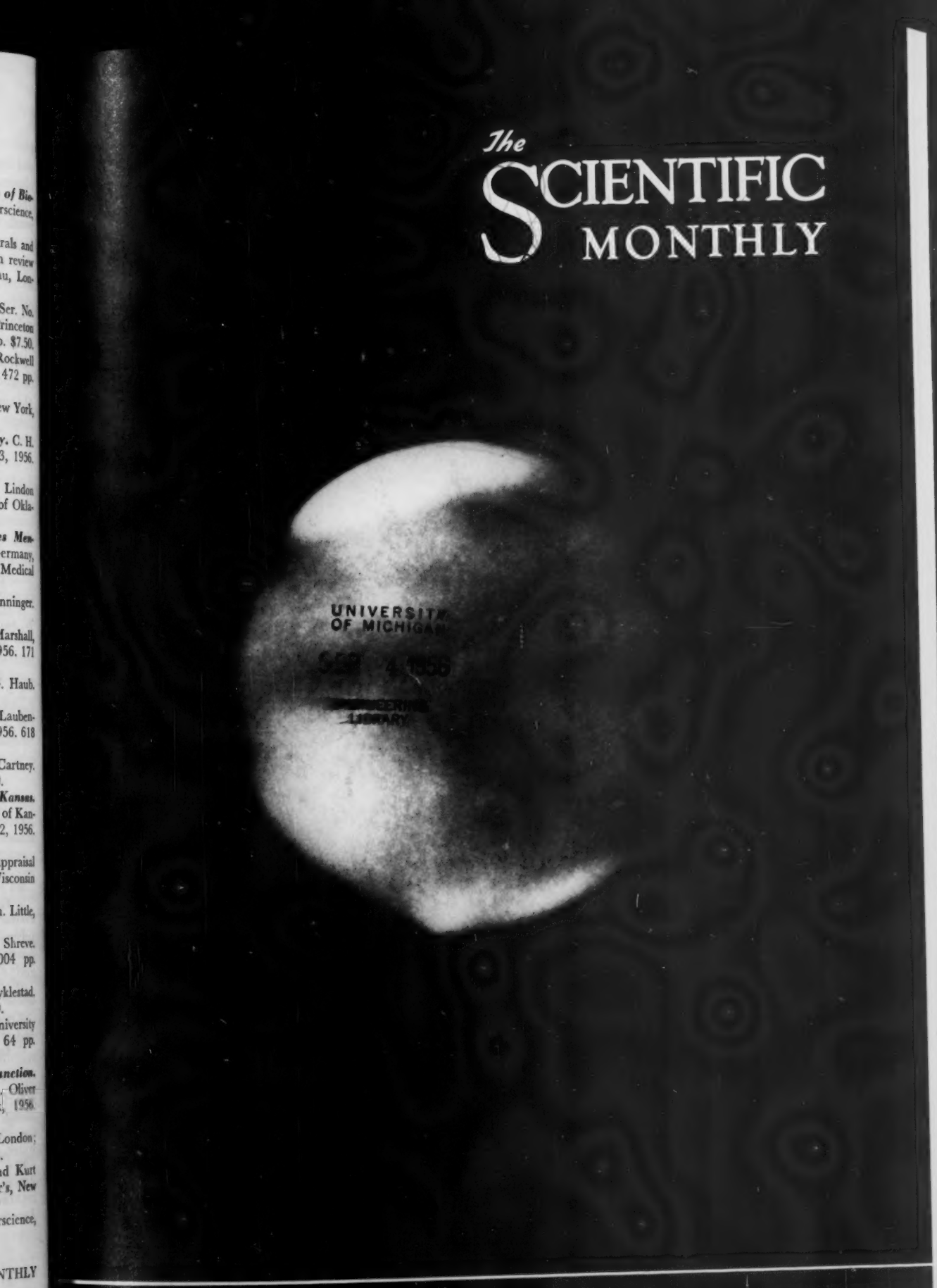

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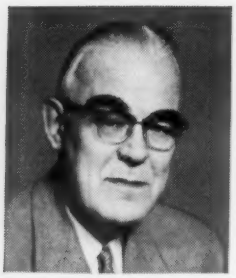
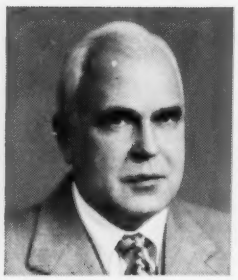
SEPTEMBER 1956

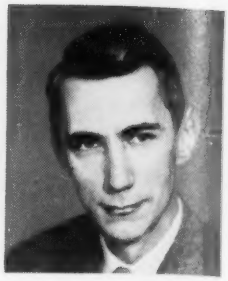
WARREN A. MARRISON. Tompion Gold Medal, Worshipful Company of Clockmakers of the City of London, for pioneer work on development of quartz crystal oscillators as precision standards of time.



AXEL G. JENSEN. David Sarnoff Gold Medal, Society of Motion Picture and Television Engineers, for Television Research; G. A. Hagemann Gold Medal for Industrial Research, Royal Technical College, Copenhagen.

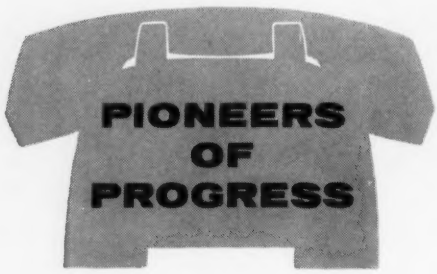


H. T. FRIIS. Medal of Honor, Institute of Radio Engineers, and Valdemar Poulsen Gold Medal, Danish Academy of Technical Sciences; important work in application of short and ultra-short radio waves.

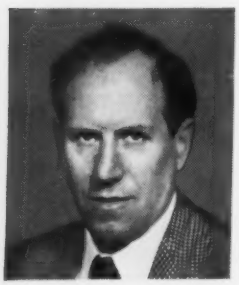


CLAUDE E. SHANNON. Stuart Ballantine Medal, Franklin Institute of the State of Pennsylvania, for contributions to a comprehensive theory of communication.

W. G. PFANN. Mathewson Gold Medal, American Institute of Mining and Metallurgical Engineers, for discovery of and pioneering research in zone melting.



H. F. DODGE. Shewhart Medal, American Society for Quality Control, for original contributions to the art of statistical quality control.



R. KOMPFNER. Duddell Medal, Physical Society of England, for his original work on the traveling wave tube.

These are some of our recent medal winners at Bell Laboratories. The awards they have won symbolize recognition for outstanding achievement in the many sciences that bear on telephony. Bell Labs is extremely proud of them—and of the thousands of scientists and engineers who work with them to keep the American telephone system the greatest in the world.

WALTER H. BRATTAIN. Co-winner with Dr. John Bardeen of John Scott Medals, City of Philadelphia, for invention of the transistor.

BELL TELEPHONE LABORATORIES

WORLD CENTER OF COMMUNICATIONS RESEARCH AND DEVELOPMENT

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[*Photograph copyrighted by E. C. Slipher, see page 161*]

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The AAAS also publishes SCIENCE. Subscription and advertising rates will be furnished on request.

Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Teflon Gland

A laboratory gland has been produced that is made of Teflon. It is chemically inert, provides a vacuum-tight seal, and may be utilized at temperatures up to 200°C when it is sealed to a vacuum of 5×10^{-6} mm-Hg. (Arthur F. Smith Co., Dept. SM, Rochester 3, N.Y.)

Scanner

A recently designed isotope scanner is used with a scaler and a collimated, directional scintillation detector to scan body areas for concentrations of radioactivity, while simultaneously producing a picture of radioisotope distribution. Commonly used to chart radioactive iodine distribution in the thyroid, the scanner may be used for delineating any other organ in the body in which a radioactive isotope is localized, and it may be set to scan a body area as large as 14 in. by 17 in. (Nuclear Instrument and Chemical Corp., Dept. SM, 229 W. Erie St., Chicago 10, Ill.)

Catalog

Items needed for determining moisture quantity in any granular, solid, liquid, or semiliquid material are illustrated and described in a new catalog. Listed are moisture tellers, speed ovens, fan-cooled desiccators, and accessory items such as balances, weights, sample pans, and drying dishes. (Harry W. Dietert Co., Dept. SM, Roselawn Ave., Detroit 4, Mich.)

Radiation Analyzer

Used in the radioisotope laboratory for gamma-ray spectrometry, a new radiation analyzer permits counting a single gamma-emitting isotope in the presence of other isotopes, identification of unknown gamma-emitting isotopes, and routine screening of radioactive tracers for determination of purity. (Nuclear Instrument and Chemical Corp., Dept. SM, 229 W. Erie St., Chicago 10, Ill.)

Unitized Distillation Apparatus

Fractional assemblies may now be constructed from standard units for different jobs. Working efficiencies up to 75 theoretical plates at 600 ml of distillate per hour are attainable. All types of distillations may thus be performed in the 50°-to-250°C range to above 350°C under reduced pressure. An entire operation is run from a single bank of interchangeable electric controls. Parts include a still head, still pot and heater, receiver, support rack, and whatever controls are needed. The same cold-finger condenser head fits all columns. It employs a swinging funnel to remove the distillate. Adiabatic conditions are achieved through electrically heated, double-aluminum shields inside the evacuated jacket of the columns. (Fisher Scientific Co., Dept. SM, 418 Forbes St., Pittsburgh, Pa.)

Thermistors

Technical data on thermistors and varistors is contained in a catalog published by Victory Engineering. The catalog includes electric and physical characteristics covering bead, disk, rod, and washer-type thermistors, varistors, thermistor assemblies, varistor assemblies, and special products such as thermistor hypodermic needles, combustion analyzers, and gas analysis cells for vapor-phase chromatography and other uses. (Victory Engineering Corp., Dept. SM, 54 Springfield Rd., Union, N.J.)

Sterilizer

Isolating sterilizers that may be mounted in a wall between sterile and nonsterile rooms have been devised. Materials to be sterilized are placed inside the instrument through a door that opens into the nonsterile room. After sterilization, materials are removed through another door that opens into the sterile area. Temperatures up to 260°C may be maintained wth adjustable hydraulic thermostat controls and recorders. (Labline, Inc., Dept. SM, 3070 W. Grand Ave., Chicago 22, Ill.)

Booklet

Construction, operation, and applications of instruments recently designed by Polarad Electronics are described in a new booklet. Discussions of a rotating-cylinder viscometer, an electron-resonance spectrometer, a recording titrator, and a Stark modulator are included. (Polarad Electronics Corp., Dept SM, 43-20 34th St., Long Island City, N.Y.)

Anti-Smog Tower

A fume-scrubbing and absorption tower is claimed by its manufacturer to be 100-percent efficient in the scrubbing of air that is saturated with sulfuric and hydrochloric acids, and 99.0 percent efficient for air that is saturated with nitric acid. Designed for the elimination of quantities of acid fumes that are discharged by chemical and industrial plants, the tower is capable of scrubbing 14 to 20 lit of evaporated acid per hour. Gas removal is carried out in three stages—absorption in liquid of the greater part of the gases, elimination of suspended globules of moisture, and neutralization of traces of gas by a chemical reagent that is contained in a tray. (Turner and Brown, Ltd., Bolton, England)

Selenium Photovoltaic

The construction and characteristics of selenium batteries that are powered by visual-band radiation are elucidated in a booklet recently published by International Rectifier. Basic circuits that are utilized in the applications for these self-generating photocells are included, as well as information on the suggested scope of such applications. (International Rectifier Corp., Dept. SM, 1521 E. Grand Ave., El Segundo, Calif.)

Noiseless Blast Burner

Pyrex glass tubing up to 75 mm in diameter and Vycor glass tubing up to 25 mm in diameter may be worked with a new Fisher silent burner. Selection of flame sizes and temperatures is made by manipulating two valves and without changing the instrument's tip. The gases are mixed and the fuel is burned externally, so that the body of the instrument remains cool. The burner is mounted on a ball-and-socket joint and maintains a pilot flame. (Fisher Scientific Co., Dept. SM, 418 Forbes St., Pittsburgh, Pa.)

Capillary Tube Rack

A new moisture-resistant rack holds essentials for microhematocrit procedures—24 capillary tubes, hemoclets, and a vial for capillary tubes. The rack contains 24 wells for unsealed tubes in upright position and 24 pairs of grooves for sealed tubes in horizontal position. (American Hospital Supply Corp., Dept. SM, Evanston, Ill.)

Alumaloy Clamps

More than 40 types of laboratory clamps that are made of a high-tensile, corrosion-resisting aluminum alloy with a tensile strength of 45,000 lb/in.^{2} and a melting point of 1100°F are described in an 8-page bulletin just published by Labline. Items are included on burette supports, utility clamps, extension clamps, 3-prong universal clamps, clamp holders, set-up frames, column clamps, and stirrers. (Labline, Inc., Dept. SM, 3070 W. Grand Ave., Chicago 22, Ill.)

Gas Content Analyzer

Quantitative determinations of carbon dioxide, oxygen, carbon monoxide, and nitrogen in blood, plasma, or other fluids may be obtained for 0.03-ml samples with a portable instrument called a microgasometer. The unit adapts the Van Slyke method to microanalyses. (Scientific Industries, Inc., Dept. SM, 132 Front St., New York 5)

Tube Heaters

Test-tube heaters, which have interchangeable blocks and aluminum reducing sleeves, for heating tubes of various sizes at one time have been produced by American Hospital Supply. Each unit has a wide temperature range. The thermostat control is variable up to 300°C and permits temperature maintenance at just a few degrees below the boiling point of reaction mixtures. (American Hospital Supply Corp., Dept. SM, Evanston, Ill.)

Fractionator

Up to 30 test tubes are held in a new fractionator under funnels in a Lucite disk that is rotated under the liquid source by a synchronous motor. The instrument collects liquid samples on a time basis that can be varied in four steps for the demonstration of changes in composition. (Edin Co., Inc., Dept. SM, 207 Main St., Worcester, Mass.)

Optochin Disks

Impregnated optochin disks make possible a simple test that differentiates pneumococcus and streptococcus bacteria. The presence of pneumococci is revealed by a zone of inhibition. Results are read after 12-hr incubation at 37°C. The disks may also be used for rapid identification of pneumococci in cultures in sputum, spinal fluids, and other organic fluids, and they remain stable when refrigerated. (American Hospital Supply Corp., Dept. SM, Evanston, Ill.)

Frequency Meter

The range of the most-used microwave frequencies, 2400 to 6500 Mcy/sec, is covered by a frequency meter with high Q performance that has been developed by Narda. Through the entire band, the loaded Q is in excess of 750 from 2400 to 6500 Mcy/sec and in excess of 1500 from 6500 to 10,200 Mcy/sec. A nomograph-type calibration chart that is incorporated in the instrument's lid, provides frequency data without calculation or written interpolation at any point in the entire frequency range to the rated accuracy of 0.2 percent (Narda Corp., Dept. SM, Mineola, N.Y.)

Portable Scintillation Counter

A transistorized scintillation counter with external probes for use in small laboratories and hospitals has been designed by Universal Atomics. The instrument operates either from line current or from four nickel-cadmium batteries. Batteries are automatically recharged when the unit is plugged into an alternating-current socket. A discriminator circuit enables the operator to determine different energy levels, such as those emanating from radium, radioactive iodine, gold, or cobalt. (Universal Atomics Corp., Dept. SM, 19 E. 48th St., New York 17)

Differential Refractometer

Featured in a new recording differential refractometer are temperature compensation and a sensitivity that is 10 times higher than that of laboratory-type refractometers. The instrument is adjustable from 10^{-1} to 4 times 10^{-4} refractive index units, and the minimum detectable change in refractive index is 1 percent of full scale at any span. Concentrations of both gases and solids in solution can be determined. (Manufacturers Engineering and Equipment Corp., Dept. SM, Hatboro, Pa.)

Nitralyzer

A continuous-reading nitrogen meter has been produced that instantaneously indicates the percentage of nitrogen in physiologic gas mixtures of nitrogen, oxygen, water vapor, and carbon dioxide. It may be used for lung-function tests, studies of time course of body denitrogenation, and physiological or clinical measurements of lung volume, respiratory dead space, and abnormalities in distribution of inspired gas. (Custom Engineering and Development Co., Dept. SM, 5103 Eichelberger St., St. Louis, Mo.)

APPLICATION FOR HOTEL RESERVATIONS

123rd AAAS MEETING

New York City, December 26-31, 1956

The list of hotels and their rates and the reservation coupon below are for your convenience in making your hotel room reservation in New York. Please send your application, *not* to any hotel directly, but to the AAAS Housing Bureau in New York and thereby avoid delay and confusion. (Members of the American Astronomical Society who wish reservations at uptown hotels should correspond directly with the Hayden Planetarium.) The experienced Housing Bureau will make assignments promptly; a confirmation will be sent you in two weeks or less. **As in any city, single-bedded rooms may become scarce; double rooms for single occupancy cost more; for a lower rate, share a twin-bedded room with a colleague.** Most hotels will place comfortable rollaway beds in rooms or suites at 2.50 or 3.00 per night. Mail your application *now* to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

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All hotels have sessions in their public rooms. For a list of headquarters of each participating society and section, please see *Science*, July 20, or *The Scientific Monthly* for August.

Rates for Rooms with Bath*

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Governor Clinton	$7.00–11.00	$10.00–13.00	$11.00–17.00	$20.00–35.00
Martinique	5.00– 9.00	8.00–14.00	8.00–14.00	16.00–38.00
New Yorker	7.00–10.00	10.00–14.00	11.50–17.00	25.00 and up
Sheraton-McAlpin	6.75– 9.75	9.75–12.75	10.75–13.75	20.00 and up
Statler	8.00–12.00	11.00–15.00	11.50–18.00	31.00–33.00

* Subject to 5% New York City tax on hotel rooms.

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Single Room ..... Desired Rate ..... Maximum Rate .....
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Twin-Bedded Room ..... Desired Rate ..... Maximum Rate .....
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(Attach list if this space is insufficient. The name and address of each person, including yourself, must be listed.)

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First Choice Hotel ..... **Second Choice Hotel** ..... **Third Choice Hotel** .....

DATE OF ARRIVAL ..... DEPARTURE DATE .....
(These must be indicated—add approximate hour, a.m. or p.m.)

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(Individual requesting reservation) (Please print or type)

ADDRESS .....
(Street) (City and Zone) (State)

Mail this now to the Housing Bureau. **Rooms will be assigned and confirmed in order of receipt of reservation.**

THE SCIENTIFIC MONTHLY

SEPTEMBER 1956

Radiochemistry

WILLARD F. LIBBY

Dr. Libby is a member of the U.S. Atomic Energy Commission. He received his training at the University of California in Berkeley. From 1933 to 1941 he taught chemistry at the University of California. In 1941 he joined the Manhattan District project at Columbia University, where he remained until 1945, when he became professor of chemistry at the Institute for Nuclear Studies of the University of Chicago. This article is based on a paper presented during the 129th national meeting of the American Chemical Society in Dallas, Tex., 10 April 1956.

RADIOCHEMISTRY consists of two complementary facets. The first is the application of chemical techniques to the problems of nuclear physics, and the second is the application of the techniques of physics to the problems of chemistry. We might regard radiochemistry, therefore, as a branch either of chemical physics or of physical chemistry.

The field of radiochemistry is broad in the sense that most chemical and nuclear-physical techniques are involved. The radiochemist may, on the one hand, have to invent a measurement technique and, on the other, to develop a new method of synthesizing radioactively labelled organic compounds. He may build a scintillation spectrometer as well as grow radioactive sugar beets. He must try to stay in the forefront of both nuclear physics and chemistry. He must know well his organic, inorganic, and physical chemistry, particularly those aspects which are likely to be of assistance in nuclear physics or are likely themselves to benefit from nuclear physics. In other words, radiochemistry is a very broad field.

Consider the great scientists who first defined this field—Otto Hahn, Georg von Hevesy and Marie Curie. They all had interests of very broad scope. Von Hevesy's work is now almost entirely in the field of biology, whereas a generation ago he was perhaps the leading physical chemist in Europe. He discovered the element hafnium and was famous for his work in x-rays and radioactivity. Hahn's fame as the codiscoverer of nuclear fission does not erase from the memory of the chemical fraternity his early notable achievements in the elucidation of the series of naturally radioactive elements, his studies in surface chemistry using radioactive tracers, and his contributions to geologic dating by the development of the rubidium-strontium technique.

The radiochemist is a manifestation of a newer demand that scientists broaden their general knowledge and techniques instead of moving toward specialization. They need not be quite a modern Leonardo de Vinci in this respect, but they must certainly be able to handle particle accelerators as well as test tubes. In the early 1930's, at the time of the discovery of the neutron and of artificially-induced radioactivity, there were a few chemists who were devoting themselves to serving the nuclear physicists and to doing whatever ordinary jobs fell their way. It was a rewarding experience because in this way they learned some of the techniques of the physicists and realized that there were opportunities for benefiting the chemical field. Thus, by

serving this early apprenticeship, lessons were learned by the Seaborgs, the Wahls, and the Kennedys who became the master craftsmen of this relatively new profession.

At the International Conference on the Peaceful Uses of Atomic Energy in Geneva in August 1955, several large sessions were devoted to the radiochemical aspects of atomic energy. It was interesting to note the large attendance at even the most complicated sessions and how spirited, interesting, and pointed the discussions were.

We know now that radiochemists have a permanent and established place in science and that they are first of all chemists, although they may from time to time conduct nuclear-physical research.

History and Contributions

The radiochemist first made his appearance immediately after Becquerel's discovery of a natural x-ray emission in some rocks containing uranium and thorium minerals. Both were elements well known at the time; their chemistry had been partially delineated, and many of their properties had been determined. But the fact that a continuing emission of radiant energy occurred from these substances was the essence of Becquerel's discovery. The dramatic story—the discovery of the uranium and thorium disintegration series, the principle of isotopy, and the discovery of the elements radium and polonium—is one of the most celebrated in all of science. It was written jointly by physicists and chemists in full collaboration.

Marie Curie certainly was one of the first and most gifted radiochemists. Her proof that several radioactive species can have identical chemical properties was, in the main, chemical in nature.

Georg von Hevesy's development of the radioactive tracer technique and his classic investigations of a number of difficult and previously insoluble problems—among which were the measurement of the self-diffusion coefficient of metallic lead and the study of exchange reactions (in which only isotopes are involved)—was a milestone in the development of radiochemistry.

Geologic time-clocks. Radioactive dating has been one of the radiochemists' principal contributions to geology and cosmology. The formation of lead by the radioactive decay of uranium-238, uranium-235, and thorium-232, in principle, affords a threefold opportunity for the determination of the time elapsed since the most recent crystallization of a mineral. The scale is in terms of thousands of millions of years, the average lifetime of these isotopes. In addition to these three isotopes, two others are of increasing importance—potassium-40, which forms argon-40 in about 10 percent of its disintegrations and has a half-life of 1250 million years; and rubidium-87 which forms strontium-87, again on a time scale of thousands of millions of years. With these five atomic clocks, it is possible to establish the age of the earth with some reliability, and it now appears that the earth and the meteorites probably are all about 5000 million years old. This remarkable result is one of the cornerstones of modern cosmology and geophysics.

These atomic clocks are almost entirely contributions of radiochemists. Other possibilities may exist, but radioactive dating is firmly established and is already a cornerstone of geophysics.

Nuclear isomers. One of the least known but most interesting contributions of radiochemistry to nuclear physics is the establishment of the existence of nuclear isomers. The principle that nuclei might have the same mass and charge, but different properties, had been recognized for some time, but in 1940 the physical existence of such nuclear isomers was first established in the case of the isotope bromine-80. It was shown experimentally, both in this country and abroad, that the bromine-80 nucleus exists in two forms, one of which decays with 4.5-hour half-life and the other with an 18-minute half-life. The proof was entirely radiochemical and consisted of the separation of the two bromine isomers chemically so that their properties could be measured individually.

It appears that the mechanism of the decay process is a gamma-ray emission by the more excited 4.5-hour state which involves nearly complete internal conversion of a soft gamma-ray emitted in the transition to the isomeric lower state of 18-minute half-life so that a K electron is ejected. This internal ionization leads to a sequence of radiations which results in the further ejection of about six additional electrons by what is known as the Auger process. The consequence is that the bromine atom produced by the 4.5-hour decay is ejected from the molecule and is chemically separable. It was found, for example, that organic bromides and bromate ion containing the long-lived isomer would grow into the short-lived daughter so that one could repeatedly milk the mother solution for the 18-minute daughter and observe its 18-minute beta decay to form krypton. The firm establishment of nuclear isomerism opened the way for the nuclear physicist to develop more general methods, and now numerous nuclear isometric states, some with lifetimes as small as 10^{-8} second, have been observed.

Hot-atom chemistry. Hot atom chemistry is concerned with a study of the chemical effects on an atom when that atom has just undergone radioactive decay. This field is distinguished from that of radiation chemistry in which the effects are gen-

eral and due to the radiation which may have been absorbed from external nuclear reactions. One of the principal hot-atom chemical effects was mentioned in the previous paragraph in connection with the discovery of nuclear isomerism—a case in which an internally converted gamma ray causes isomerization by the Auger process. A more general type of hot-atom effect is that due to the recoil of an atom that has emitted a particle or a radiation in a nuclear decay process. The best known reaction of this type is the well-known Szilard-Chalmers effect.

Szilard and Chalmers showed that the neutron irradiation of ethyl iodide to produce radioiodine resulted in the separation of a fraction of the product radioiodine from the target ethyl iodide. A considerable fraction of the radioactive iodine so produced could be separated by extraction with water. The mechanism proposed was that the radioiodine was ejected from the ethyl iodide molecule by the recoil from the gamma rays emitted following the neutron capture by the normal iodine atom. It was calculated that about 200 electron volts of recoil energy would be transmitted to the iodine atom. These enormous recoil energies impart to the radioactive iodine atom properties which are entirely unusual in chemistry, but which are chemical nevertheless.

In the course of extensive investigations in the subject of hot-atom chemistry, it has been established that there are some generalities. One knows that there is a considerable tendency for recoiling atoms to substitute for atoms of nearly the same mass—that is, radioactive ethyl iodide is formed. All of the radioactive ethyl iodide formed in the Szilard-Chalmers experiment was the result of reformation and not to failure of dissociation in the initial act. That is, the radioactive ethyl iodide was formed by a radioactive atom's displacing a neutral iodine atom in the molecule, and not by simple neutron capture within the ethyl iodide molecule. This fact has been established by showing that the irradiation of ethyl iodide vapor results in no production of radioactive ethyl iodide. The radioiodine is found to be deposited entirely on the walls of the vessel and to be in water-soluble form.

The chemistry of the recoil iodine atom is such that it has a large tendency to substitute for iodine rather than for hydrogen and carbon atoms. It is postulated that this is due to the requirement that the momentum of the recoiling atom be transferable in essentially a single collision in order that ethyl iodide be formed. This requirement, of course, means that the target atom must have nearly equal mass. If, in the collision of an iodine atom with another iodine atom, the momentum is transferred successfully, the radioactive iodine atom will be left with small residual energy in the vicinity of the site originally occupied by the iodine atom which was struck in the collision, the result being a tendency to substitute and reform ethyl iodine.

This type of substitutional process can occur even at high velocities, the probability of the residual energy after the collision being small enough to allow the iodine atom to collide successfully with the radicals left by the expulsion of the nonradioactive iodine atom being proportionately less for higher energies of recoil. For example, let us consider the energy required for an atom to escape from the collision site by pushing aside the surrounding molecules. If we assume this energy is 1 electron volt, a recoiling atom with 100-electron-volt energy would be left with small enough residual energies in only 1 percent of the collisions with nonradioactive iodine.

As the iodine atom proceeds along its path, however, and fails to make head-on collisions with nonradioactive iodine atoms, it will collide with hydrogen and carbon atoms and thereby lose energy. Eventually it will reach a velocity at which the chemical bonds between the carbon and hydrogen atoms and between the carbon and iodine atoms are no longer negligible relative to the recoil energy. Momentum can then be transferred successfully from the recoiling radioactive iodine atom to the ethyl iodide molecule as a whole, or to portions thereof. This range of energy is called the "epithermal range." In this range, general substitution of hydrogen and carbon atoms and of all the constituent groups of molecules becomes possible.

The experimental proof of the distinction between (i) the high velocity or hot reactions in the main portion of the track of the recoiling atoms and (ii) the reactions characteristic of the epithermal portion is demonstrated by the large difference in the effects of phase transformation on the yields for the two types of reactions. It is found, for example, in the case of propyl bromide that hydrogen substitution reactions are three or four times as probable in solid propyl bromide as they are in the liquid phase, whereas the bromide substitution reactions are increased not more than 50 to 75 percent. It would seem that the epithermal reaction is much more sensitive to the differences between the solid and liquid state than are the reactions in the high-energy range because of the smaller energy densities involved.

In recent times, the application of hot-atom chemistry to organic synthesis has become quite practical. It has been shown by Wolfgang and Rowland at Brookhaven National Laboratory that radioactive hydrogen—tritium—can be substituted into organic molecules in this way. In order to expose the organic molecule to the recoiling tritium, they have physically mixed the target material with

finely divided lithium carbonate and irradiated the slurry. The recoiling tritium atoms formed by the lithium in the lithium carbonate have sufficient range to escape from the particles of lithium carbonate and transfer a considerable fraction of the tritium to the organic phase. The latest accomplishment, I am told, is that they have brought about the substitution by direct acceleration of the tritium ions using the organic phase as a target. High specific activities have been obtained in this way.

Radiation chemistry. A little more than 40 years ago, Samuel C. Lind undertook the investigation of ionized molecules from a chemical point of view by using radon as a source of ionization. He arrived at many very important results and published an extensive series of papers with Glockler and others which have served as the foundation for the subject of the chemical effects of ionizing radiation.

In the present era of the atomic age, we find his results to be a matter of extreme practical importance. We are concerned, for example, with the decomposition of water in radiation fields in many important ways. Some types of nuclear reactors require that water be exposed to very intense radiation fields, and there is considerable resulting decomposition. It is a remarkable fact and a very fortunate occurrence that the atomic hydrogen and hydroxyl radicals so produced recombine with such efficiency that these reactors are practical. Reactor engineers may be inclined to take it as a bit of good luck, and I suppose this is true, but it is certainly due to our knowledge of the radiation chemistry of water.

One would hope that an analogous understanding of other possible moderator materials might permit us to short-circuit extensive testing programs and lead us more quickly to their applications to atomic power. Why is it that aromatic hydrocarbons seem to be more resistant to radiation than other organic molecules? Are we at all clear concerning the difference in the effects of the ionization—that is, the radiation chemical effects and the reactions due to fast neutrons, the hot-atom chemistry or Wigner effects? There is a great deal of confusion in the minds of people who are considering possible moderator materials and materials to be used in radiochemical processing about the basis for the effects of radiation on matter. Further understanding and investigation of these subjects will be extremely useful; they will lead us along the sure path of good fortune and help us to discover more substances for use in pile technology which are comparatively inexpensive and which have the desirable nuclear properties of water.

Radiation chemistry also has already shown that a great deal can be done in the use of the free radicals generated by radiation for the induction of polymerization reactions. It is now known that plastics formed by chains initiated by gamma radiation may have superior properties. The cross linking induced in this manner appears to yield products obtainable in no other way. We may in this manner have arrived at one of the important chemical uses of the atom. Radiation chemistry is going to become and in fact already has become a subject of real practical importance.

New elements. Perhaps the most remarkable of all the achievements of the radiochemists is the discovery and production of the transuranic elements by McMillan and Seaborg, and particularly by Seaborg and his coworkers in recent years. The only one of these new elements which has proved to be of direct practical importance is plutonium, because of its fissionable isotope plutonium-239, but the story of the study of the chemical properties of all of these elements is fascinating and inspiring. Because these elements were produced in extremely small amounts, many of their chemical properties were determined by esoteric radiochemical methods. Seaborg has shown that they are members of the 5*f* rare-earth series and, therefore, in the main favor the trivalent state. He has shown that the delicate factors controlling the equilibria of the cation-exchange resins are smoothly varying functions of the chemical characteristics which can apply only to a bona fide rare-earth series. It is fully demonstrated that the quantum mechanics can handle the atomic system of 100 or more electrons with confidence. That is, that the theoretical prediction of the occurrence of the second rare-earth series is really correct. The value of this knowledge is inestimable, and even though no other practical use than the fission of plutonium should ever be made of these elements, all of the work of the able men involved will have been amply repaid by their contribution to our basic knowledge of chemistry and physics.

Radiochemical Training for Chemists

Having discussed what radiochemistry is and what it has done, I should like to turn now to a second very important point—the virtual necessity of radiochemical training for all chemists. It would be regarded as unthinkable to attempt to teach the life sciences without the use of a microscope. Training in radioisotope techniques should be obtained consecutively in the freshman, sophomore, and junior years of college. In this way, experience in the application of radioactive materials would become a part of the student's continuing education.

I should like to see on the reagent shelf in the standard freshman chemistry laboratory radioactive chemicals placed beside the conventional ones and

distinguished from them by a small asterisk in the appropriate symbol in the formula. For example, there should be on the shelf bottles of radiocalcium chloride, calcium radiochloride, hydroradiochloric acid, radiosulfuric acid, sodium radiosulfate, radiochlorine, potassium radiopermanganate, radioferrocyanide, radioferricyanide, ferroradiocyanide, and ferriradiocyanide. In general, nearly every chemical on the shelf could well have its radioactive counterpart. There would be the necessity merely of being sure that the radioactivities were so feeble as to be perfectly safe in all respects.

How can this be managed? Well, the isotopes are now available. I hope, therefore, that some enterprising professors of freshman chemistry or teachers of high-school chemistry will undertake to stock their shelves with these radiochemicals and use them in their courses. The rewards will be great. Let us consider in more detail how the instruction should proceed.

Laboratory experiments and techniques. It is necessary to use detection instruments of special construction with these ordinary chemical isotopes. It seems to be a law of nature that the isotopes of longer life time have softer radiations—so soft that they will not penetrate the walls of ordinary geiger counters and other measurement instruments. For example, radioactive hydrogen or tritium (12.2-year half-life, average energy 5 kilovolts is so feeble that it is necessary to measure it in the gaseous state. Radiocarbon (5568-year half-life) emits radiation with average energy of about 50 kilovolts, which can be stopped by a piece of ordinary paper. The half-thickness amounts to about 2 milligrams per centimeter of solid matter, or a little less than 1 inch of air. Radiosulfur (3-month half-life) has an energy similar to radiocarbon; radiosodium-22, with a 3-year half-life, has an energy about 3 times that of radiocarbon, but is still so soft that counters with especially thin walls are necessary. And so it is with most of these long-lived useful isotopes that can be kept on the shelf for a period of months or years without replenishment.

The type of detection instrument necessary for this measurement is easily built and is used with commercially available counting equipment. It consists essentially of a geiger counter with a wall made not of metal but of plastic sheeting which is metalized to make it conducting. The best sheeting consists of the new plastic, Mylar, which has a thin gold deposit vaporized on it. This material is available commercially. In this way, a counter wall can be made by wrapping the thin sheeting (with the conducting surface inward) around plastic disks which form the heads of a cylinder. The counter wire is strung along the axis of this cylinder. The ends of the disks must be firmly anchored since the plastic sheeting cylinder wall has no intrinsic strength.

It is also true that the plastic wall is not gastight, and it is necessary therefore continuously to pass an appropriate gaseous counting mixture through the assembly from a gas cylinder. One of the best gases for this purpose, which is commercially available in small cylinders under pressure, is helium with 1 or 2 percent of *n*-butane added. If attention were given to the avoidance of gross leaks, such a cylinder could furnish a classroom with counting gas for at least 1 year. The counters are inflated by the gas flowing through them, and the extremely thin walls offer maximum sensitivity for the detection of the softest beta radiation. In fact, of all the isotopes mentioned, only tritium would not be sufficiently penetrating.

These counters are so rugged and simple in construction that no difficulty should be encountered in operating them. If one becomes contaminated or if the wall is pierced by accident, it is a simple matter to mount a new plastic sheet as a wall. The students simply place precipitates in close contact with the wall, to obtain maximum sensitivity.

A convenient counting arrangement can be obtained by painting the precipitate on the inside of a second cylinder which fits closely around the counter (the cylinder would have to be split into two pieces so that this could be accomplished). The effective geometry of this arrangement would be quite high.

Relative measurements of the radioactivity of two or more samples suffice in perhaps 75 percent of the cases. However, primary standards for most isotopes useful in the high-school or college laboratory can be obtained from the National Bureau of Standards, and *simulated* long-lived standards are available commercially for such short-lived isotopes as iodine-131 and phosphorus-32. The student compares the standards with the unknown under identical conditions and by simple proportion obtains a measure of the absolute counting rate of the unknown. Under conditions of high-school or college use, an accuracy of ± 10 percent in the absolute sense is readily obtainable.

For these significant isotopes it is possible, fortunately, under the conditions necessary for the use of the smallest amounts of radioactivity, to conduct absolute assays of all the radioactive materials measured on a routine basis. In other words, the freshman chemistry students can take a bottle of radiosulfuric acid, make a precipitate of calcium or barium sulfate from a known volume of the acid solution, using excess calcium or barium reagents, wash and dry the precipitate and by placing a known area of the precipitate in close contact with the wall of the thin-wall counter just described,

calculate immediately the absolute number of radioactive sulfur atoms per cubic centimeter of the sulfuric acid solution. The reasons that this can be done are as follows.

Most absorption curves for beta radiation are complicated. A plot of the count rate versus the thickness of absorber yields a line which is curved even on logarithmic paper. It appears that the cause of this complicated curvature is twofold. (i) Many beta activities consist of more than one spectrum—that is, the transition is a mixture of two transitions to two different states and would yield a nonexponential absorption curve even if the pure beta spectrum characteristic of a single transition did give one. (ii) Beta radiation consists of electrons which in traveling through ordinary matter are extensively scattered.

It therefore is necessary, in order to obtain a simple absorption law, that the scattering be eliminated by placing the sample close to the counter wall so that the scattering effect is standardized and the variability caused by the exact placement of the absorber is removed. It is very fortunate that the isotopes which are long-lived and most useful in the chemical laboratory in general involve single transitions with pure spectra and do show exponential absorption curves under conditions of close geometry. This holds for tritium, radiocarbon, radiosodium (except for the gamma rays involved), radiochlorine, radiophosphorous, radiosulfur, and so forth. For all these important beta radioactivities, the absorption curve, under conditions where the sample is placed close to the counter, is a straight line on a plot of the logarithm of the count rate versus absorber thickness.

This exponential absorption law means that for thick samples the effective thickness is the reciprocal of the absorption coefficient, or 1.44 times the half thickness. For example, for radioactive sulfur the effective thickness of calcium radiosulfate would be 3.6 milligrams per square centimeter. If the sample were placed close to the counter so that the geometry factor might approach 2.5, corresponding to 40-percent interception of all radiation emitted by the samples, the observed count rate should be multiplied by 2.5 and then these counts could be attributed to 3.6 milligrams of precipitate for each square centimeter of calcium sulfate area. From these data, the absolute radioactive sulfur content of the calcium sulfate is immediately calculable as is, of course, that of the sulfuric acid from which it was derived.

It would be necessary to correct for the absorption of the radiation in the wall of the counter. This would amount to about 35 percent since the plastic wall has a thickness of about 2 milligrams per square centimeter.

Thus the ratio between the actual count rate recorded and the number of disintegrations per minute would be 4.6, and if the area of the sample were 100 square centimeters—which could well be the case if a sizable precipitate were painted on the inside of a cylinder which fits closely around the counter—one would have some 360 milligrams of precipitate effectively recording with the known efficiency of 22 percent. The counter necessary to accommodate 100 square centimeters of precipitate might be 1 inch in diameter and 5 or 6 inches long. Unshielded, a counter of these dimensions would normally have a background of perhaps 200 counts per minute. If it were shielded, the background would be reduced to perhaps 75 counts per minute. The students could well be expected to use a shield, but it might be that in a crowded laboratory it would be easier not to shield the counters but to use sufficient radioactivity so the shielding would be unnecessary.

Let us calculate the amounts of radioactivity needed in the radioactive reagents. Taking sulfur again, and supposing that a count rate of twice background or 500 counts per minute would be desirable, then at 22-percent efficiency the 360 milligrams of precipitate should contain 2300 disintegrations per minute. If the precipitate were calcium sulfate, this would be equivalent to 2.6 millimoles of sulfuric acid of a radiosulfur content of 880 disintegrations per minute, per millimole, or for a 1-liter bottle of 6-normal sulfuric acid 2.6×10^6 disintegrations per minute of radiosulfur, which is 1.2 microcuries. At the current rate, this is worth ¼ cent, so the cost of the isotope is negligible. Similar calculations apply to the other isotopes, although chlorine-36 is more expensive than normal because of its long lifetime. Chlorine-36 at the same specific activity as the sulfuric acid would cost $1.20.

Availability of Radioisotopes

On the basis of an average classroom of students using the counter described, the annual cost of the radioisotopes need not be more than $10 to $20, including shipping and handling charges.

The U.S. Atomic Energy Commission has been interested for some time in making radioisotopes and labeled compounds available in small quantities. It has removed administrative restrictions on acquiring and transferring so-called "exempt" (research) quantities of materials and has encouraged their distribution. Nine commercial companies are now engaged in such practices; some of them offer "isotope kits" at rather nominal prices.

A few years ago, the AEC, in cooperation with the public school system of the City of New York,

published a booklet entitled "Laboratory experiments with radioisotopes for high school science demonstration," which is for sale by the Superintendent of Public Documents, Washington, D.C., for 35 cents. The AEC, with the help of the National Science Foundation and the Oak Ridge Institute of Nuclear Studies, has developed a unique program to assist secondary schools in science teaching. A selected group of high-school teachers will undergo an intensive 3-month training period in Oak Ridge and then spend 9 months traveling about the country giving lecture demonstrations in science classes at individual high schools. This is a good beginning.

Why Teach Radiochemistry?

With these facilities, the teachers of freshman chemistry could conduct many of the classical experiments in a more interesting and enlightening manner. But the principal benefit would be that the student would have a continuing use for his knowledge of radiochemistry just as he has a continuing use for his slide rule. Radiochemistry would not come as an adjunct but as an integral part of his science education, and his own ingenuity could be brought to bear earlier in the development of new applications of radiochemistry. He might become more interested in chemistry as a career because of the stimulation of new applications and of new facts naturally afforded.

In organic chemistry courses, particularly the laboratory preparatory and analytical courses, the possible applications of radiocarbon are of tremendous variety. It is possible to measure radioactive liquids with the counter I have described by thoroughly mixing them with an inert substance such as talc and then counting as described. The absolute specific activity of the moistened talc is then determined, and with the knowledge of the relative weights of powder and liquid, it is immediately possible to calculate the absolute specific activity of the liquid.

Similarly, the synthesis of an organic compound with radiocarbon incorporated into one of the reagents involved would allow a study of the mechanism of formation of that compound to be made by the student of elementary organic chemistry. For example, ethyl acetate might be synthesized with radioactive ethyl alcohol or, alternatively, with radioactive acetic acid, and a determination of the specific activity of the product could be made to confirm the equation for the esterification. The synthesis of iodoform from radioactive acetone carbon-labeled either in the methyl position or in the ketone position might be studied by measurements of the specific radioactivity of the iodoform and other products obtained. There are countless other possibilities such as those described by Richard H. Robinson in the July 1955 issue of the *Journal of Chemical Education*.

At the present time, the principal peaceful applications of atomic energy that have been realized are the uses of radioisotopes and of nuclear radiation. Atomic power undoubtedly will be developed eventually and will be invaluable, but we should remember that at the moment radioactive isotopes and radiation have already established themselves as very important benefits of the atomic energy project. In fact, it seems likely that even if we were not to attain atomic power, the use of isotopes alone would repay the American taxpayer for the enormous investment he has made in establishing the atomic energy laboratories and the production complexes. We estimate that at present American industry and agriculture are benefiting to the extent of several hundred million dollars per year in savings and profits of one sort or another due to isotopes and radiation. In addition, of course, are the priceless benefits to the health of the nation.

How can we benefit as rapidly as possible? It will not be possible to maximize the good things the atom is bringing us without teaching radiochemistry to all chemists. We should do this, and we should begin immediately. The general improvement in the understanding of nuclear phenomena which would follow such an educational policy is of vital importance to the nation's welfare. The most difficult of the problems which the Federal Civilian Defense Agency faces is one of education concerning the effects of atomic weapons, and of these effects, perhaps the most difficult to describe are the effects of radioactive fallout. The understanding of nuclear phenomena which would be gained by the students of chemistry in the colleges and high schools of the country if a policy of general radiochemical education were adopted would be invaluable in spreading this knowledge and in increasing the rate at which people in general and public officials in particular would learn to come to some judgment of their own about the nature of the hazard and what steps could well be taken to protect the populace in the case of atomic war.

Finally, with the rapid growth of the atomic energy industry and with the increasing importance of all of the effects of the peaceful uses of the atom, it seems to me that it is a matter of importance that at least some general understanding of this new force be obtained by everyone—the citizen, the investment banker, the lawyer, and the administrative official—that they may properly assess and judge the many important issues of our times.

Cattle Raising: a Way of Life in the Venezuelan Llanos

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VENEZUELA, which is about 1½ times the size of Texas, falls naturally into four regions (Fig. 1), each differing from the others in terrain, climate, soils, natural vegetation, and even in the ways the people live. This nation is occupied by slightly more than 5 million people, but they are not distributed uniformly. Most of them dwell in the Northern Highlands. The Guiana Highlands and the Llanos are but sparsely settled; in fact much of the Llanos is unused. In those portions that are put to use, cattle raising prevails. Some petroleum is also being taken from the region.

This article is concerned solely with the Llanos (which at one time was the most important part of the country) and with its cattle industry—past, present, and future. Cattle raising has proved to be the most persistent and permanent form of enterprise, having survived for 400 years and outlived all other temporarily more profitable enterprises. Moreover, it is regarded as a traditional Llanos way of life (Fig. 2).

What the Region Is Like

The Llanos del Orinoco, which comprise a huge geographic region partly in Venezuela and partly in Colombia, are said to occupy some 130,000 square miles. The region is one of low elevation, nowhere exceeding 1,000 feet above sea level, and the many sluggish rivers that cross it wind about in broad valleys.

Botanically, the Llanos are regarded as tropical savanna—not a region of uniform aspect as Humboldt pictured it, but rather one of infinite variation. Savannas are grasslands occurring in areas that do not receive enough rainfall over a long enough period to support an important growth of trees. To be sure, here and there a single tree or even a clump may be seen, but grass—thousands of miles of it—is the predominant vegetation. The climate consists of a rainy season and a dry season. It is the long dry period, lasting from December to April and sometimes even into May, and the high temperatures and the rapid rate of evaporation typical of the tropics that hinder the growth of forests. Along stream courses, however, gallery forests do grow. According to Schimper, the most important factors determining the occurrence of savanna are climatic (*1*); Richards, on the other hand, believes that the climatic theory is difficult to uphold (*2*); and Myers regards the Llanos as a fire climax, although he believes that soil conditions (of which actually little is known) may also be important, as well as heavy overgrazing (*3*). Myers says, "I have never seen in South America a savannah however small or isolated or distant from settlement which did not show signs of more or less frequent burning" (*4*). The llaneros, believing that firing vegetation improves the grass and kills cattle ticks, deliberately set frequent fires. Grass of course recuperates, but trees are killed.

During the season of heavy rainfall, the rivers swell tremendously, overflow their banks, and inundate enormous areas. It is believed that more than a third of the Llanos is inundated at times. The rainy season is later followed by months of hot, dry weather that burns up the vegetation. Thus the Llanos is alternately sun-baked (Fig. 3) and drenched (Fig. 4).

Settlement

Even before white men came to the New World, the Llanos were largely empty, for the small Indian population lacked the knowledge of irrigating and draining the land, and nearly all our present-day domestic animals are importations from the Old World. Hence grazing was unknown. Those who dwelt in the region occupied only those lowlands where there was sufficient moisture to grow crops and where the permanent rivers supplied fish.

The first livestock were taken into the Llanos about 1548. Within a century the number of the cattle had increased to about 140,000 head, and by 1812, under the care of llaneros, the number had increased to an estimated 4.5 million, only to drop to a mere 256,000 11 years later. The herds have always suffered during wars. Thus, in the bitter campaign for independence from Spain, private ownership of Llanos livestock was to all intents and purposes disregarded, for Bolívar had to have something to use in payment for the exorbitantly priced war supplies that he purchased on credit from American, British, and Dutch merchants (*5*). Following wars—for example, the Five Years War (1866–1870) and the many civil wars—the cattle industry invariably would rebound, only to toboggan again with resumption of hostilities. In short, the diagrammatic portrayal of the changes in the number of Llanos cattle over a period of years resembles a modern stock market chart.

One of the brightest interludes for the Llanos cattle industry was the 18 years following 1870 during which Guzmán Blanco ruled Venezuela. Guzmán Blanco was no ordinary caudillo. Among other things, he encouraged the improvement of agriculture and stock raising. He introduced better animals and attempted to teach the llaneros to take better care of them. However, when he was finally forced into exile in 1888, virtual anarchy followed. The number of cattle dropped precipitously (Table 1).

General and President Juan Vicente Gómez, who ruled most of the time as president and always as despot from about 1908 to 1935, strongly influenced the cattle business. He and his family and friends gradually gained possession of large tracts of the better lands in the Valencia Basin, where the emaciated cattle from the Llanos were fattened before they were slaughtered. Gómez forced both foreign and Venezuelan ranchers out of business. Taxes became so high that other ranchers could not compete with this man who paid the taxes on his own lands and herds to himself. Although the ranches he acquired were gained by legal means, it was always at bargain prices. If an owner did not wish to sell, Gómez found ways to force him to do so.

During part of the colonial period, in the more accessible parts of the Llanos where slaves were held, indigo, tobacco, and cacao were grown. However, with the abolition of slavery and the loss of the closed Spanish market, agriculture became unprofitable and virtually died out. Today probably not more than 0.1 percent of the region is under cultivation. The best authorities on the Llanos believe that only those parts of the river valleys that

Fig. 1. Major regions of Venezuela. 1, Maracaibo Basin; 2, Northern Highlands; 3, Llanos; 4, Guiana Highlands. Transportation is the crying need throughout the entire country. The construction of railways and highways is costly and difficult, but the government's policy of "sowing the oil" is increasing the mileage of both. Nonetheless, no adequate surface transportation facilities tie these four regions together.

are of sufficient elevation to be preserved from floods and certain higher foothill areas in the states of Barinas, Portuguesa, Cojedes, Guárico, Anzoátegui, and Monagas have any future whatsoever in agriculture (*6*). The steady emigration of numerous families and individuals from the Llanos, which have a limited population, to the more healthful coastal and mountain towns and cities is significant. Actually, depopulation has been going on in the Llanos since the wars with Spain.

Cattle Industry Today

The cattle population of Venezuela is presently estimated to be 5,631,986 head. Two-thirds of these animals are in the Llanos. Considering the enormous size of the region, this is a low carrying capacity, which indicates (i) that the browse is not very good; (ii) that the region has striking inherent weaknesses; and (iii) that the business has not been profitable.

Cattle ranches on the Llanos are large, averaging about 3000 acres; many, however, are much larger. With modest exceptions, the proprietors, many of whom are absentee landlords, take little interest in improving their property, pastures, or animals. Some have installed wells, windmills, and water troughs and are improving their stock by inter-

Fig. 2. Cattle drinking at an artificial pond in the Llanos, State of Guárico. Many such ponds are needed to sustain the animals during the dry season. [Courtesy of D. Vila C.]

breeding with Zebu bulls (Fig. 5). If it is possible to generalize accurately, one may say that for the most part the work with cattle is done in an inefficient and wasteful way.

The *criollo* or "native" cattle, which predominate over the Llanos, are descendants of European cattle brought into the region centuries ago by the Spaniards, and they have changed little during the past 400 years (Fig. 3). Since they have received little care, they have for the most part become resourceful, hardy, and self-reliant; they have also become semiwild, and they breed without control or management over the unfenced Llanos. They subsist wholly on what nature provides; usually they know when, during high water, to move toward elevated ground and, in low water, to seek the areas near the perennial streams. Moreover, they have learned how to protect themselves and their calves against the wild animals that prey upon them. They have also become relatively resistant to the pests and the local diseases that flourish largely uncontrolled.

They are small, long-horned, slow maturing, and varicolored. They do comprise an excellent foundation animal, however, upon which to build crossbred types. The crossbreeds yield a higher percentage of edible meat when dressed; this is important, for the reproduction efficiency (the number of calves raised annually per 100 cows) of Llanos cattle is extremely low—less than 50 percent. Zebu cattle have a higher heat tolerance than European cattle, and this carries over to the F1 crosses. The aim is to blend the good qualities of the *criollo* cattle with the heat resistance of the conditioned breeds from India, producing a new animal suited for the heat of the Llanos. Actually, as the amount of Brahman breeding is increased in a strain of cattle, the heat tolerance becomes greater (*7–9*). Such animals graze longer during the day and keep in better flesh. In addition, cattle with light coat colors and short hair are best suited to the tropics. Both Brahman and *criollo* cattle possess short-hair coats and are quite well adjusted physiologically to the climate.

In order to improve the quality of its cattle, Venezuela is importing from the United States about 5000 breeding animals annually. The breeds consist of the United States Brahman (a new type of Brahman), Santa Gertrudis, Charbray, Brangus, and Beefmaster, all of which were developed to thrive in the hot, humid American South (the Cotton Belt). It appears certain that they will meet successfully the rigid climatic requirements of the Llanos.

It is worth noting that all these cattle, which contain Brahman blood, have been bred for large size, for it is well known that cattle in the tropics become smaller with each new generation (*10*). All are also good rustlers—a requisite for Llanos cattle because of the seasonal oscillation from hot, seared to flooded land. They are also resistant to cattle ticks and other parasites and pests that are

so difficult to control in the tropics. Finally, they are better able to withstand the long, difficult walk to market. Llaneros assert that British breeds can hardly make 10 miles per day, whereas those with Brahman blood can do 20 miles without trouble and without much loss of beef.

No presentation of the Llanos cattle industry, however brief it may be, should fail to discuss the role of the seasons. During the rainy months (May to November), the rivers overflow their banks, inundating extensive areas and thereby restricting markedly the size of the grazing area. The cattle must then seek what high ground there is and make for the margins of the foothills and low mesas that stand like islands above the flood. Occasionally, the waters rise too rapidly for the animals to get away and they have to make the best of a bad situation (Fig. 4). At this time, there is some loss to alligators, electric eels, jaguars, and the ferocious and voracious fish, the piranha (*Serrasalmus natteri*). Traveling in schools and fearing absolutely nothing, these fish set upon man or beast if either falls or gets into the water. They can skeletonize a steer in 10 minutes. It is understandable why they are the most respected and feared fish in the world.

The same lands are later stricken by a dry season; the rivers recede rapidly and the merciless sun burns up the countryside. At this time the ground bakes very hard. The grasses suffer not only from the burning of the sun but also from insufficient moisture and the dry northeast winds.

Fig. 4. The Llanos is drenched in the wet season or *invierno* (winter). [Courtesy Consejo de Bienestar Rural]

The grasses become so woody and high in fiber that they are unpalatable and unnutritious. Hundreds of cattle die from sheer hunger and thirst at this time. Those animals that do survive lose weight and are in run-down condition; these are the major reasons accounting for the late age of marketing Llanos cattle—4 to 6 years. The poor quality of the grass is also a reason why the llaneros have burned it for centuries, insisting that almost immediately the green tender shoots come up in the burns. H. H. Bennett, former chief of the U.S. Soil Conservation Service, appears to agree: "At any rate new grass does come in within about 10 or 12 days after a fire and the cattle everywhere head out of the old grass into the burns. They are

Fig. 3. The Llanos is sun-baked in the dry season or *verano* (summer). In the vernacular, the rainy season is called "winter" despite the fact that the region lies in the Northern Hemisphere, which then has its summer. [Courtesy of Creole Petroleum Corporation]

Table 1. Number of cattle in Venezuela in various years from 1804 to 1950 (*15*, p. 43).

Year	Head
1804	1,200,000
1812	4,500,000
1823	256,000
1828	956,859
1833	2,437,150
1839	2,086,724
1847	5,503,000
1858	12,000,000
1864	5,800,000
1873	1,389,802
1876	2,158,267
1883	2,926,733
1886	5,275,481
1887	6,687,041
1888	8,476,291
1899	2,351,590
1910	1,461,557
1937	3,090,661
1950	5,631,986

reported as gaining weight as a result of the change from grass nearly as tough as wire to grass which for awhile is relatively tender and palatable" (*11*).

This does not mean that burning the grasses is a good thing; it destroys the humus and microorganisms in the soil. It has also resulted in a diminution of the better grasses: thus, over much of the eastern Llanos, inferior grasses, which are more resistant to elimination by fires, prevail (*12*). However, the Venezuelan government is taking measures to do something about this matter, for the country needs more beef; more and more people have been moving into the cities. The oil companies and those mining iron ore are also interested in improvement of the grasses, for they would like to have the Llanos supply meat and milk. One introduced perennial that appears to offer considerable promise from current experiments is kudzu. It provides good pasturage, has high feeding value, and is available for pasturage at any time of the year. Burning should be tolerated only as a means of controlling the worst weeds and the thorny plants which otherwise are almost impossible to exterminate.

The reader may be wondering why man has not improved the native Llanos grasses as American prairie farmers and Argentine pampas farmers have done. The answer is largely based on climate and soils: the Llanos are tropical wet and dry in climate and hence are subjected to alternate inundation and drouth. Accordingly, only coarse, hardy grasses can endure such severe conditions. Such improved grasses as Guinea, Para, Napier, Guatemala, and Natal do well only on fertile, well-watered areas, which occur in the Llanos only in small valleys close to streams. Most of the soils of the Llanos are notorious for their infertility, and no really suitable grass has yet been found that can thrive over most of the region with its climatic extremes.

The scarcity of water over most of the Llanos during the dry season, particularly where rivers are scarce, is a major deterrent to successful stock raising. Often cattle have to travel many miles to find drinking water. Obviously they lose much weight on these long walks under the hot tropical sun. It is not an uncommon sight to see Llanos cattle trying to drink in mud holes where dead cattle are already bogged down. Such mud holes are contaminated and contribute greatly to the spreading of diseases (*13*, p. 8).

When no water can be found at all, the cattle involved die of thirst, and hundreds die annually. During the past several years the government has been taking measures to correct this situation: it has been drilling wells and constructing artificial ponds for storing rain water (Fig. 2). Thus in 1953 it was responsible for drilling 531 artesian wells and for constructing 53 ponds. The ponds must be sufficiently large to last through the long dry season, for the rate of evaporation is great.

The health of Llanos cattle would be improved if salt were made more easily available. It is not uncommon to observe cattle licking alkaline banks near roads (*13*, p. 12).

Cattle Drives

Periodically the llaneros round up their steers and bulls and head them for market, driving them

Fig. 5. Brahman cattle on a ranch or *hato*, State of Guárico, in the Llanos. This breed is being introduced more and more because it can withstand heat and insects and fatten under the trying conditions of browse and water. It also stands the walk to market with negligible loss of weight. [Courtesy of D. Vila C.]

Fig. 6. End of the trail: *Criollo* cattle arriving in Maracay, center of the nation's fattening area, after the long drive from Apúre in Central Llanos. The hard surface road in the picture should not delude the reader into believing that the animals traveled this way on the entire trek: most of the trip was over a long, hard, grueling trail. [Courtesy Institute of Inter-American Affairs]

on the hoof (Fig. 6). Those destined for Caracas, the best market (with the largest number of people able to pay high prices for beef) are 4 to 5 years old, the rest 4 to 10. Only 7 percent of the total cattle population of Venezuela are slaughtered annually as compared with 21 percent in Argentina and 33 percent in the United States.

This trek, long and difficult, taking from several days to several weeks, depending upon the distance, is over dusty, rocky, and brushy country and along rugged trails often without feed and on some days with little or no water. The animals usually reach their destination weak, gaunt, and lame. Losses are appreciable; loss in weight alone is placed at 20 percent of the herd.

Unfortunately the government has a regulation setting a minimum live weight at which cattle must be slaughtered. This regulation is based on false principles, for it leads to the wasteful practice of carrying steers up to ages of 4 to 6 years and slaughtering them in thin condition. In those countries with the most advanced livestock industries, the average carcass weight tends to be lower than it is in Venezuela. Venezuelan beef is not of high quality.

At the ends of the trails—Maracaibo, Valencia, and Maracay—there are pastures where some of the animals are rested and fattened. Large numbers, however, are slaughtered on natural savannas without any special fattening. More and more cattle, however, are being slaughtered in the Llanos. There are slaughtering plants at Calabozo, El Rostro, El Tigre, San Cristobal, San Carlos, Valle de la Pascua, and elsewhere (Fig. 7), whence beef is flown daily to the large cities. Caracas, with a population of 1 million inhabitants (1956 estimate) now receives one-fourth of all its meat by air as sides of beef (*14*). Some is even hauled by truck when road conditions permit, but the major problem of the Llanos is the paucity of modern land transportation. Roads are poor in the dry season and virtually nonexistent in the rainy season (Fig. 8). However, the government realizes that transportation is the greatest single problem confronting it and hence is allocating a very considerable part of the national budget for road construction. According to the Master Highway Building Program, Venezuela needs 14,430 miles of main highways but has an existing mileage of only 7475. Nearly all the hard surface roads lie in the highland region (Fig. 1) around and connecting the cities of Caracas, La Guaira, Maracay, Valencia, Puerto Cabello, San Felipe, Barquisimeto, and Guanare. Few good highways extend into the Llanos, although some of the very important cattle districts in the State of Guárico are connected with the cities in the northern highlands by a first class highway.

Summary and Conclusions

It is believed that the Llanos in the early days presented conditions for grazing that were superior to those encountered today. The grasses were more nutritious and they remained green for a longer period. There were no systematic annual burnings

of the grasses. Floods were less violent, for then the mountains were clothed with trees; they had not yet become subjected to serious soil erosion by deforestation for making charcoal, for farming, and for grazing by goats. Since the land supported 12 million cattle in 1858 and only slightly more than 5½ million in 1950, it would appear that these generalizations are sound. In addition, from 1850 to 1856 the annual exportation of cattle hides averaged from 500,000 to 600,000 in number. This figure, combined with the number of hides consumed by the domestic leather industry, indicates that nearly twice as many cattle were slaughtered annually than now (*15*, p. 44).

It has been noted that there are many obstacles to successful cattle raising in the Llanos: the lack of sufficient grass of good quality for pasturage all the year around; the scarcity of water during the dry season, combined with extremely high temperatures and steady dry winds; the prevalence of insect pests, parasites, and diseases; lack of control methods (the cattle graze at will since no fences separate one ranch from another); inadequate livestock credit; an antiquated method of handling meat; backward government pricing methods; and above all the absence of a modern transportation system. Of these, climate cannot be altered; this means that alternate floods and drouths must continue to be contended with. There seems little hope today of introducing superior grasses on a large scale, although it is reported that kudzu has done well where it has been introduced and given good care. The problems of water, insects, management practices, improvement in the livestock, expansion of the market, and credit all appear to be conquerable. Even now some progress is being made in all of them. Good highways promise ultimately to tap key centers in the

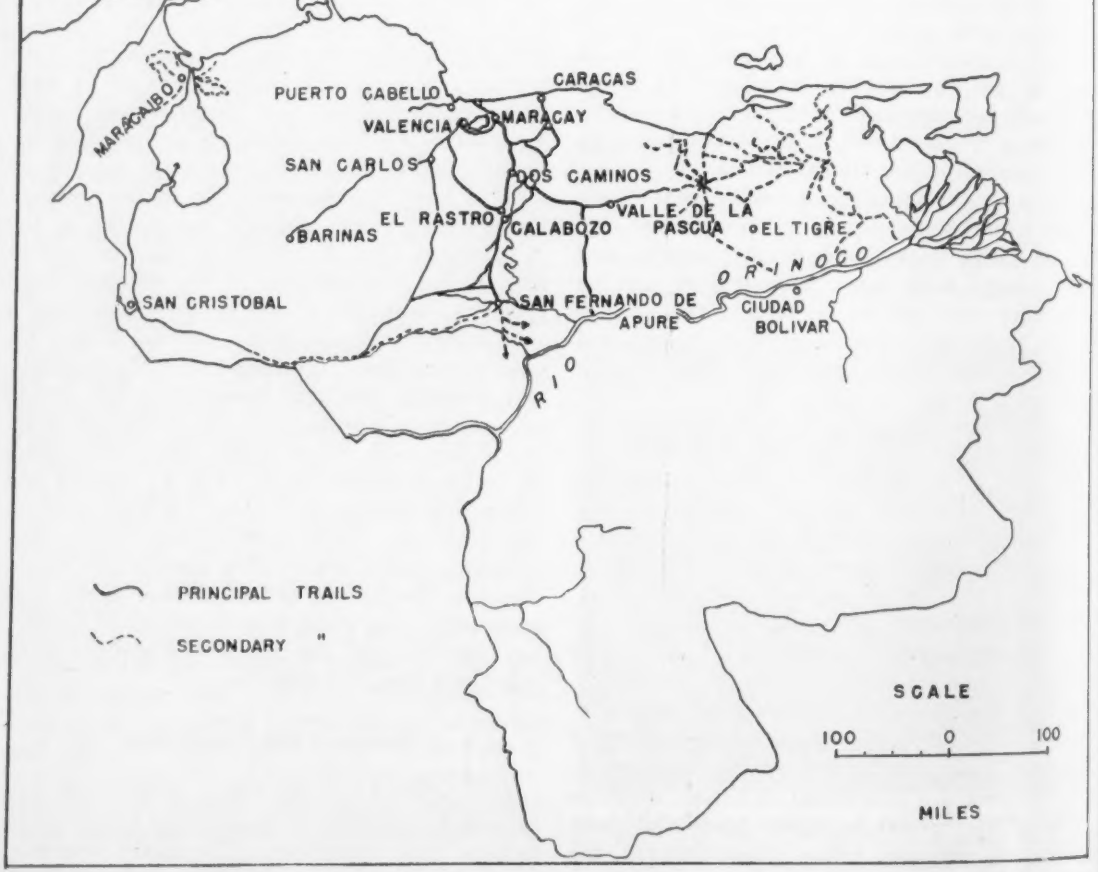

Fig. 7. Cattle trails and principal slaughtering centers in Venezuela. The importance of the Llanos to the country's cattle industry is here apparent.

Fig. 8. During the rainy season, roads in the Llanos are either impassable or nonexistent. Hence movement is by dugout. This photograph was taken near San Fernando, State of Apúre. [Courtesy Consejo de Bienestar Rural]

region. On the other hand, there are two almost insuperable obstacles: (i) the lack of incentive on the part of the llaneros and (ii) the poor economic yield from keeping cattle. "The potential productivity of our cattle industry has always been beyond what the market was absorbing. Therefore, there was no stimulus for its development nor any object in promoting it" (*15*, p. 45).

Today, cattle raising is a way of life rather than a genuine economic enterprise. It has been said, possibly too pessimistically, that if these cattlemen were moved by money incentives, which hold for much of the rest of the world, the industry would come to an end tomorrow. If one adds to these deficiencies the remoteness of the region, which cuts it from contact with the populous northern highland and the outside world, one can understand why the Llanos is backward and neglected.

Can the Llanos be expected to become one of the world's leading commercial cattle regions? The answer appears to be in the negative: from the viewpoint of the world's better cattle lands, the region appears to be among the poorer pastoral areas. Actually, the Llanos are misnamed when they are called "natural pastures." Hence, in terms of the near future, the Llanos should be counted out as an area suitable for cattle grazing on a large scale. Nonetheless, Venezuela is one of the most progressive countries in all Latin America and hence some progress is to be expected. In fact, substantial improvement in the market for beef and in prices being paid for it is resulting in a back-to-the-cattle movement. How far it will go and how important the cattle industry will become only time will tell.

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I believe that the time given to refutation in philosophy is usually time lost. Of the many attacks directed by many thinkers against each other, what now remains? Nothing, or assuredly very little. That which counts and endures is the modicum of positive truth which each contributes. The true statement is, of itself, able to displace the erroneous idea, and becomes, without our having taken the trouble of refuting anyone, the best of refutations.—HENRI BERGSON.

Pseudonyms of Physicists

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WHEN one considers that the growth of science depends on the communication of ideas and that scientists reap some measure of satisfaction by signing their written works, there would appear to be little occasion for the use of pen names by scientists, be they physicists or others. Yet pseudonyms have enjoyed more than a little popularity, and it is interesting to conjecture on the reasons why an author hides behind another name. The pseudonyms that have been used may be grouped according to the motive that led to the suppression of the author's name. Although one can discover a variety of possible reasons, operating singly or at times in concert, the motive generally stems from some form of diffidence or fear of the consequences.

Classification

As an example of timidity and hesitation, there is the case of William Thomson (later Lord Kelvin) who came up to Cambridge at the age of 16 to study under the famous tutor William Hopkins. Not fully occupied by his studies, or playing the cornet in the musical society and rowing on the Peterhouse College crew, he found time to do original work, and during his four years as a student he published ten articles in the *Cambridge Mathematical Journal*. Representing as they did his first mathematical and scientific contributions, they were submitted with some diffidence, at least at first, over the pseudonymous signatures "P.Q.R." and "N.N." Another, and possibly more important, reason for hiding his identity undoubtedly lay in his father's fears about the propriety of a young undergraduate's writing advanced papers—one, indeed, even before he entered Cambridge. That first paper concerned "Fourier's expansion of functions in trigonometric series," and, since the respected mathematician Philip Kelland had expressed the thought that "everything in Fourier was wrong," it must have been felt, particularly by a father who we know had such high ambitions for his son, that it would be unseemly and unpolitic for young Thomson to take a contrary position (*1*).

As for fear of reprisal, such a motive may be attributed to P. G. Tait when he signed his letters to the editor of *Nature* with the pseudonym "G.H." (*2*). And well he might have, despite his good intentions, for, in taking exception to a contributor's definition, "The energy of heat is made up of heat and temperature," he in turn was assailed in the same periodical (*3*) as "somewhat of a beginner in science" and as one "so proud of his acquaintance with certain elementary propositions in thermodynamics, that he is on the *qui vive* to detect in others an ignorance of them."

On the other hand, anonymous and pseudonymous literature by scientists may be divided into classes according to the method by which concealment is sought. There are (i) the strictly anonymous; (ii) those pseudonyms in which the author refers to himself by a description; (iii) those in which the author adopts a false or partly false name, which as often as not is fantastic; and (iv) those in which he uses initials.

Robert Boyle issued one edition of his *Skeptical Chymist* in complete anonymity presumably because of his well-known modesty and because he knew the work would create something of a stir in which he hesitated to become involved (*4*). But he favored descriptive phrases more. In all humility, he signed himself "A person of quality," "A fellow of the Royal Society," and "A person of honour," the last being used both for his "Curiosities of chemistry" and "The martyrdom of Theodora and Didymus." Other examples are Humphry Davy's use of "An Angler," Edmund Halley's use of "An Infidel Mathematician," and David Brewster's assumption of "A Calm Observer" on taking up a nonscientific subject. A complete list would indeed be a long one.

Then there are false names, and for them Greek and Latin offered favorite sources. Thus Boyle used "Pyrophilus" and "Philaretus" to write on nonscientific topics, the latter presumably to signify that the love of truth was a basic trait of his character. The botanist Robert Brown used "Verus." Appropriately enough, the mathematician Abraham Sharp assumed "Philomath" and the zoologist Edward Blyth took "Zoophilus." And

Simon Newcomb chose "Aristides" when he competed at the age of 59 for a prize of $150—which he won, incidentally—for an article entitled "The elements which make up the most useful citizen of the U.S." (*5*).

We may think of a name as partly false if it bears some relation to that of the author, obscuring but not completely concealing. Some writers retain all or some of the elements of their names but rearrange the components in such a way as to require recasting on the part of readers who desire to identify the writer. Perhaps the best known case of the use of a partly false name, at least in the popular literature, is that of the 19th-century mathematician, Charles Lutwidge Dodgson, who wrote the ever delightful *Alice's Adventures in Wonderland* under the pen name "Lewis Carroll," a reversed modification of "Charles Lutwidge" corresponding to the German "Carl Ludwig." Oldenburg, the first secretary of the Royal Society of London, signed his name "Grubendol," illustrating a simple, though impure, plan based on a reversal of the spelling.

Pascal, the 18th-century natural philosopher, also used anagrams. He signed himself at different times as "Amos Dettonville," "Louis de Montalte," and "Salomon de Tultie," all anagrams of one another though unrelated to his real name. While admiring his ingenuity in rearranging the letters to produce very plausible names, we must recognize that the large number of letters made the task easier than otherwise. The latter two anagrams were used in his nonscientific work, while "Amos Dettonville" was employed at a time when, having solved the heretofore refractory problem of the quadrature of the cycloid, he offered prizes for its solution. After the appointed period had elapsed, no one had furnished a complete answer, and Pascal himself then published his own solution under the pseudonym (*6*).

Fantastic pseudonyms might well be classified under false ones. Yet they deserve a place all their own. Whereas William Osler's use of "Edgerton Y. Davis" is an example of a false name (he signed it on hotel registers and said he "never but once met the man"), his "Obadiah Tweedledum" illustrates the use of a fantastic pen name (*7*). Similarly we can so describe the use of "Rednaxela Gnimelf" by Alexander Fleming, the Scottish mathematician; of "Harry Bluff" by Matthew Maury; of "Peter Puzzlem" by Sir William Herschel writing in the Ladies' Diary; and the use of "Ben Yamen" by Benjamin Pierce, the eminent American mathematician (*8*).

Finally, initials are often used, though the order may be changed or one or more suppressed. Robert Boyle, besides using "R. B.," once used his finials

A caricature of Lord Kelvin by Spy. [From *Vanity Fair* 29 (1897).]

"T. E." (combined with "A layman"), when he wrote on "Some considerations about the reconcileableness of reason and revelation." Or with his initials an author may associate others taken from words which indicate his profession, social rank, or some other characteristic, as Boyle did in signing himself "T. H. R. B. E. Fellow of the Royal Society."

Chief Varieties

A further classification of pseudonyms would be scarcely worthwhile (*9*). One can note many a subspecies and many motives therefor, especially in the 19th-century literature. Were one to select any one case as exemplifying the chief varieties, one would no doubt choose that of Thomas Young, who used a large number of pseudonyms—probably more than any other physicist—and did so for an uncommon reason (*10*).

Thomas Young possessed almost as many interests as pen names. To physicists he is known princi-

pally for his work on the wave theory of light, to physiologists for his pioneering studies of the human eye, and to antiquarians for his contributions to the translation of the Rosetta stone. It was these many diverse interests that led to his taking of pen names. Sometimes he used them to sign his less erudite articles; at other times he employed a pen name when he contributed to that very respectable periodical, *The Philosophical Magazine.*

The most plausible reason for his use of pseudonyms must have lain in his known desire to maintain his medical practice. Much as he enjoyed his non-medical pursuits, he wanted to conceal from his colleagues, patients, and prospective clients his many diverse interests, obviously demanding time and energy, which would be construed as unseemly and interfering with the serious practice of medicine. Moreover, he may have wished to avoid a repetition of the ridicule he suffered at the hands of Henry Brougham in regard to his Royal Institution lectures. Indeed, after Brougham's attacks—and before Young used any pseudonyms—we find him saying "With this work my pursuit of general science will terminate; henceforwards I have resolved to confine my studies and my pen to medical subjects only."

This he was fortunately unable to do, and in all he disguised himself with 14 different names, excluding his *Encyclopædia Britannica* pen name, which he used 23 times, always in scientific writings. Some are easy to decipher, as "T. Y." and "F. R. S. L." Others have no special meaning, as "A B C D," "A. B. C. D., " "E F G H," and "X X X X." (They probably represent "T. Y. M. D.") Seven articles were published anonymously. A number, however, have a more obscure source. There is "Emeritus," used when Young was but 34. Was it chosen to signify his departure from the professorship of natural philosophy at the British Institution? So we may conjecture, for he left the institution only a short time before he used the name. And there are "Hydrophilus," "Apsophus," "Dytiscus," and Hemerobius." Whence came their inspiration? Probably from the subject matter considered under each pen name since one can point out etymological connections between the adjective pseudonyms and the topics discussed in the four contributions to *Nicholson's Journal.* The pseudonym "S. B. L.," used four times, can be ascribed to Young's work on the Rosetta stone. The three letters played a key role in the translation and they appear as his signature in 1820 and 1821 after his success with them. One may think he signed them with pride. But there is no ready explanation for his use of "O. R." to sign certain articles in the *Encyclopædia Britannica,* although it has been pointed out that men whose last initials were "O" and "R" were writing for the same publication.

In any study of pseudonyms, one soon observes that many are so fantastic that they may be regarded as having been adopted by way of a joke, as indeed they often were, sometimes for their owner's private entertainment, sometimes for the amusement of his friends. Shame, fear of consequences, and timidity were not the motivating forces. Rather we see in certain pseudonyms and shorthand notations an expression of the writer's penchant for wit and his appreciation of the droll phrase, a manifestation of *joie de vivre,* as it were.

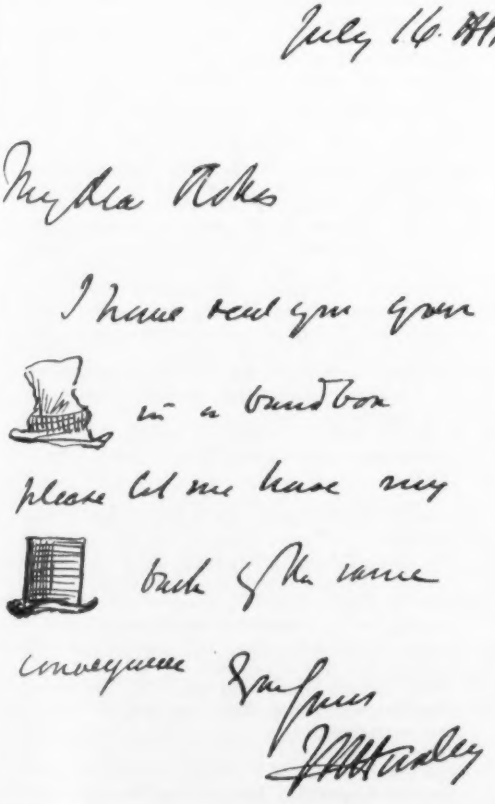
The letters of Peter Guthrie Tait, William Thomson (Lord Kelvin), and James Clerk Maxwell reveal an extensive list of pseudonyms used partly for their own amusement and partly to save time during an extensive exchange of letters (*11*). Their names, used in salutation, body of the letter, and signature were often written in simple code letters. Thomson signed his letters "P Q R" (in early years) or "T." Tait addressed Thomson simply as "Dear T" or "O T." In turn Tait was addressed as "Dear T'" and signed himself "Yrs T'." The heavy correspondence was carried on mainly in connection with a book they were collaborating on (*Treatise on Natural Philosophy*) and upon the book's publication it too became known as "T and T'." The practice of using "T and T'" persisted from 1862 until 1892, when Thomson was knighted and became Lord Kelvin. At this point in their correspondence, one comes across this shorthand acquiescence, "O T' O K K." Once when Tait wrote to Thomson about Tyndall, he used for Tyndall "T''." Their good friend Hermann Helmholtz was designated as "H²" (*12*).

Tait, the central figure in the correspondence, was a keen devotee of golf, often playing as many as five rounds a day at St. Andrews, the first as early as 6:30 A.M. He was very proud of his son Freddie, often his golfing companion, who gained national prominence as a youthful competitor but at an early age was killed in the Boer War. With his son doing the driving, he measured, indoors and using a ballistic pendulum, the speed of the ball as it was struck by the club, and as long as his son was playing Tait studied the physics of golfing, using Bernoulli's principle to explain carry, slicing, and pulling. Enraptured once by the qualities of a certain club, he wrote in praise a five-verse poem which he signed "G. H.," the pseudonym he also used with articles in *Nature.* It is thought to represent "Guthrie Headstone." We can account for "Guthrie" easily; "Head" we can associate with "Tait" (French *tête*); and the "stone" can be interpreted "Peter." Thus, Peter Guthrie Tait.

Shorthand

Furthermore, Tait liked to use phonetic spelling and other short cuts in his writing. Of course he used the obvious "R.S.E." for Royal Society of Edinburgh and "D. T." for Dynamical Theory of Heat, but more amusing is his "diff^1 = "" and "anser." Once he wrote Thomson, whom he often had to cajole into working on their book, "When do you return? U must come to I, or I 2 U, as soon as possible, for there is very much pressing work." Again, writing late at night, he says, "Having come to *Inertia,* I shall go to bed forthwith."

In such shorthand Maxwell was not to be outdone by Tait, who in 1860 had won from Maxwell the professorship of natural philosophy at Edinburgh University. Both before and after this bit of rivalry they were staunch friends and carried on a correspondence full of wit and frolicsome allusion. Maxwell, too, used "T and T'" freely, and added "H" for Hamilton and "d p/d t" for his own signature. He once wrote inquiringly to "T'" in a language Tait must have enjoyed. "O. T'. R. U. AT 'OME?" Maxwell also used "Ice" for "once," "4 nion" for "quaternion," "θ Δ c s" for "thermodynamics," "= potls" for "equipotentials," "x q s" for "excuse," and "E Qq D [tails]" (*13*) for "excuse details."



Letter sent by T. H. Huxley to G. G. Stokes in 1887 asking for an exchange of hats. [From J. Larmor, *Memoir and Scientific Correspondence of the Late Sir George Gabriel Stokes* (Cambridge University Press, Cambridge, 1907), p. 176.]

Puns

Again and again we find these men using puns in their letters. As they used the pun, it is an injustice to refer to it as the lowest form of humor. Actually, the pun is not a form of humor, it is a form of wit, and it is intellectual in quality, as opposed to humor, which is emotional. The pun requires a great deal of facility for association both for its conception and for its comprehension. Lamb and Shakespeare used it extensively. It is said that Lamb was one day accosted by a friend who said, "Lamb, make me a pun." "Upon what, Sir?" was Lamb's reply. Writing to Thomson on 30 March 1863, Tait said in a postscript ". . . and let me see Digitalis on Newton . . . before a fortnight is out . . ." This we can interpret as a complex pun: digitalis = poison = Poisson, a well-known mathematician.

Lord Kelvin's humor was said to have been elementary. Yet he sometimes introduced German into his plays-on-words. When asked about the work of Hittorf on cathode rays, Kelvin replied that he had "hit it off" very well. About a paper by the German Starke, he commented that "he did not think it at all strong" (*14*).

From all we can learn, the great Maxwell was fond of the pun, the double-entendre, and anagram. As a youth he liked puzzles and riddles, talked incessantly, and in many ways showed the workings of a very active mind. At the age of 15, to him a butterfly was a flutterby. When he was 18, Maxwell wrote to Lewis Campbell (his future biographer) of Wheatstone's stereoscope which he says was described at the Brightish Association for the Advancement of Science. Later he frequently poked fun at the association and its members by the use of various abbreviations. Maxwell also enjoyed situation humor. He possessed a huge top which he used to illustrate many principles of dynamics. This top he brought with him to Cambridge when he came up for his M.A. degree in the summer of 1857, and one evening he exhibited it to his friends in his room. It was heavy and possessed very little friction, and when his friends left, the top was still spinning. The next morning, Maxwell, noticing one of his friends of the night before approaching his room, jumped out of bed, set the top going, and retired again. Needless to say, the top commanded increased respect thereafter (*15*).

Verse

Maxwell is known too for his amusing versifying, a pastime shared by Sir W. R. Hamilton and J. J. Sylvester, among others. In one of his poems he shows the penchant of T and T′ for thrift with words. He had written to "T" only to find that Kelvin had been away from home helping lay the Atlantic cable. This inspired him to write several verses beginning with the two that follow:

2 (U)
Mark how the telegraph motions to me,
2 (U)
Signals are coming along,
With a wag, wag, wag;
The telegraph needle is vibrating free,
And every vibration is telling to me
How they drag, drag, drag,
The telegraph cable along.
2 (U)
No little signals are coming to me,
2 (U)
Something has surely gone wrong,
And it's broke, broke, broke;
What is the cause of it does not transpire,
But something has broken the telegraph wire
With a stroke, stroke, stroke,
Or else they've been pulling too strong.

In a footnote to the poem Maxwell adds, "To avoid repetition let (U) = *Under the sea* so that 2 (U) . . . represents two repetitions of the sentiment." When he sent some of his verses to Tait, he added the heading, "For P. G. Tait but *not* for *Ebony,*" meaning by *Ebony* the very popular *Blackwood's Magazine.*

His own name Maxwell signed "d p/d t," not simply in some of his correspondence but even to some poetry which appeared in *Nature.* This is equivalent to his initials, for a well-known equation of thermodynamics reads d p/d t = J C M, where J is Joule's equivalent, C is Carnot's function, and M is the rate at which heat must be applied to increase the volume of a gas per unit while its temperature is kept constant. An earlier signature was used when he was 11 and wrote his father from the Edinburgh Academy, a school for boys, about the happenings there. He signed his name "Jas. Alex. McMerkwell," an anagram for James Clerk Maxwell. On another occasion, since his initials were the same as those of his father, Maxwell signed his name, "J. C. M., No. 2."

Of Maxwell, his biographers, Campbell and Garnett, said ". . . He had a strong sense of humor, and a keen relish for witty or jocose repartee, but rarely betrayed enjoyment by outright laughter." It is indeed clear that his humor was of the mind as much as of the heart. In his various pseudonyms, puns, and puzzles he found a delight not often to be shared by the nonscientist. Not only had he all the associations engendered by his wide acquaintance with the literature of his own country —he was especially fond of reading aloud from Shakespeare—but he had, moreover, all the associations available from physics and mathematics. Words, yes; and symbols too.

What fortunate men were T′, T, and that prince of punsters, d p/d t!

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8. For certain of these I am indebted to Gale Bortermann.
9. One might, however, institute a special category for pseudonyms shared by two or more persons. There is at least one known case of the same pseudonym having been used on separate occasions by two different writers, and there is a splendid example of two collaborators using a single pen name. Balfour Stewart and P. G. Tait became interested in theology and together wrote a book to give their views wider audience. Named the *Unseen Universe,* the book eventually went through several editions, including a French translation. It was announced in a curious way by a 94-letter anagram published in *Nature* (1874) and signed "WEST." which, being translated, means "WE S[tewart] T[ait]." One could also gather pseudonyms under the category of scientific terms. Thus, John Robinson Pierce, a member of the staff of the Bell Telephone Laboratories, writes at times under the names "John Roberts," "J. J. Coupling" (inspired no doubt by his study of spectroscopy), and "E. M. Field," the initials of which represent adjectives modifying "Field."
10. A. Wood and F. Oldham, *Thomas Young, Natural Philosopher* (Cambridge Univ. Press, Cambridge, 1954).
11. C. G. Knott, *Life and Scientific Work of Peter Guthrie Tait* (Cambridge Univ. Press, Cambridge, 1911).
12. The Tait, Thomson, Maxwell, (and Hamilton) correspondence was matched—in pseudonym signatures —by that of a coterie headed by Sir Oliver Lodge, famous for his early work on electromagnetic waves. Lodge signed his name "Λ," while his brother, Alfred Lodge, was designated as "λ" (note the lower case); George Fitzgerald was "ϕ," and George M. Minchin, with whom Lodge carried on a very extensive correspondence, was known as and signed his name with a combination of the Greek letter gamma and the English "M." See Sir O. Lodge, *Past Years, An Autobiography* (Hodder and Stoughton, London, 1931), p. 143.
13. For *tails* Maxwell drew some short, wavelength lines.
14. Lord Rayleigh, *Proc. Phys. Soc. London* **48**, 221 (1936).
15. L. Campbell and W. Garnett, *The Life of James Clerk Maxwell* (Macmillan, London, 1882), p. 499.

Features of the Solar Spectrum as Imposed by the Physics of the Sun

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IT is clear that the major problems underlying terrestrial utilization of solar energy lie more in terrestrial technology, physical and biophysical, than in purely astronomical studies of solar phenomena. In a rough sense, the primary astronomical datum necessary is the solar constant, or the solar energy flux incident on the earth's outermost atmosphere. This quantity measures the integrated energy available for nonselective solar power sources—the "horsepower per square yard in Arizona" that is so often quoted.

In a more refined sense, one requires the spectral distribution of this solar energy, particularly for selective power sources. We consider first the integrated radiation.

Thanks largely to the long-range studies (*1*) of the Smithsonian Institution, the solar constant is well-specified. By far the greatest portion of the energy is concentrated in the ordinary optical spectrum. The primary uncertainty in the absolute value of the solar constant that was obtained from the Smithsonian work lies in the two-stage process of correcting their spectrobolometric observations for the ultraviolet and infrared spectral regions that lie outside the observed region, which included wavelengths from 3460 to 24,000 angstroms. On the one hand, there is the correction for the radiation received by the pyheliometer but not the spectrobolometer; on the other hand, there is the correction for the spectral regions, particularly those below 2900 angstroms, that are received by neither instrument. For reference, we note that these corrections amount to some 9 percent of the total; or, alternatively, 91 percent of the solar energetic output lies in the wavelength band just given. In addition to the Smithsonian reports proper, an excellent summary of the problem by Johnson (*2*) may be consulted for details.

The current Smithsonian value, corresponding to the earth's mean distance from the sun, is 1.95 calories per minute, per square centimeter. Niccolet suggested that a revised consideration of the infrared correction leads to a slightly higher value, 1.98 (*3*). Shackleton and Quirk, using total solar radiation measures from balloons, supported a slight increase over the Smithsonian value, obtaining 1.96 (*4*). They reported that the accuracy of the balloon measures was ± 1.5 percent, so their results are quite compatible with Niccolet's suggested revision as well as with the Smithsonian value. Johnson rediscussed the whole problem, obtaining increases in the ultraviolet correction to the spectrobolometer, and to both the ultraviolet and infrared unobserved portions of the spectrum. His final value is 2.00 ± 0.04, which is somewhat larger than the other values, although the spread lies within the indicated accuracy. One should be careful to note that the difference between Johnson's value and that of the Smithsonian studies lies in a rediscussion of the corrections to be applied to the measured data, and so the ± 2-percent limit of error does not apply to the difference in values. However, this limit does apply to the difference between these values and that obtained from the balloon measures.

For the terrestrial solar-energy application, such correction to the extraatmospheric value is academic. The value of interest is that actually observed at the earth's surface. For this application, the variable effect of atmosphere and clouds becomes all-important. The astronomer either tries to eliminate these effects by not observing on cloudy days, or he tries to correct for them by observing at variable zenith distance and, as I have mentioned, by introducing data on the unobserved portion of the spectrum from other sources. Since

these features—transmission through the clear and cloudy atmosphere—lie in the province of others, I retreat to the astronomer's province outside the terrestrial atmosphere.

Solar Variability

The question of intrinsic variability of the solar constant has been much discussed. The absence of any large-scale change in the solar energetic output is well-established by geologic and climatologic data that cover 1000 million years or so. Most of our information from direct measures again comes from the work (*1*) of the Smithsonian Institution, particularly that of C. G. Abbott. In an altogether too-brief summary of a painstaking amount of work, two aspects should be noted. First, any periodic variability in the observed solar constant, if present, seems small—less than some 3 percent. Second, there is a good possibility that such variability, if present, arises from periodic variability of the terrestrial atmosphere rather than from intrinsic solar variability in the ordinary optical spectum. These two aspects are strengthened by the recent work of Hardie and Giclas on the differential method of studying solar variability by comparing planetary and stellar luminosities (*5*). Observations over the years 1949–1954 fail to indicate a solar variability exceeding 0.5 percent in the optical spectrum. A comparison between the small fluctuations found and those reported by the Smithsonian observers over the years 1953–54 fails to show any correlation between the two sets of measures. All these measures, of course, refer to the ordinary optical spectrum.

Again, we note that for the terrestrial use of solar energy, the question of whether any variability present arises intrinsically or atmospheric-wise is academic. What is important, in direct application, is the small size of the effect. Again, the relevant discussion must turn to the relative importance of such effects against that of atmospheric cloudiness. On the other hand, for indirect application the possible existence of the variability has, for example, strong climatological importance, and here the solar-energy interest merges with that of the astronomer and upper-atmosphere physicist.

From the standpoint of the solar astronomer, a well-established variability in solar constant arising from a variability in the state of some part of the terrestrial atmosphere would be essentially as interesting as an intrinsic solar variability in the optical spectrum; for such variability in properties of the terrestrial atmosphere must ultimately have some cause, presumably solar. Thus the problem would become that of a spectroscopically localized variation in the solar energetic spectrum rather than a general one. Indeed, while the summary I have given would preclude a large change in total solar emission, there remains the possibility of a major change in a particular spectral region, whose contribution to the integrated radiation is small, but whose effect on the state of the atmosphere is appreciable. Thus, if there is indeed a true variability in the measured solar constant, one has a choice between a small-amplitude intrinsic variation in the total solar radiation, or a larger-amplitude variation in a localized spectral region. These two alternatives have of course quite different implications with regard to both their solar origin and their terrestrial effects. Again in a highly condensed summary, one can say that the paths of investigation leading from the concept of an intrinsic variation in integrated radiation have been very unrewarding. On the other hand, a number of investigations show results quite consistent with the notion of variation over limited spectral regions, both periodically and sporadically. Some physical feeling for the implication of such remarks may be gained if we make a brief inquiry into the relationship between the distribution of energy in the solar spectrum and the thermodynamic structure of the sun.

Solar Energy Spectrum and Solar Structure

The basic question we now ask is: What part of the sun need we consider in a discussion of the solar energy spectrum? That is, we ask whether we may restrict ourselves to a limited region of the sun, or whether it is necessary to treat the whole solar structure to clarify the points raised in our discussion of the solar constant. Here we must be very cautious and define carefully what we mean. From an observational standpoint, the solar energy spectrum is that which we see—that is, that which emerges from the sun. Clearly the photon or particle emission reaching us directly must come from a level relatively high in the solar atmosphere. The actual observed energy spectrum represents a summation over all emitting layers in the solar atmosphere, diminished for each layer by the absorption in both the overlying layers in the solar atmosphere and in the various absorbing layers between the sun and the terrestrial observer. Thus, if our observational accuracy were ϵ, it would seem reasonable to consider only those layers for which a fraction larger than, say, 0.1ϵ of the emitted photons or corpuscles reaches the terrestrial observer. Clearly, ϵ depends on wavelength. In specifying the geometrical extent of the "relevant" layers of the sun, we must then choose that extent corresponding to the wavelength of greatest transparency.

If we express these remarks algebraically, the

situation becomes more clear. Along some direction that makes an angle θ with the solar radius, we expect the emergent specific intensity to be

$$I_\nu(\theta) = \Sigma j_\nu(\theta; x, y, z)\rho(x, y, z)e^{-\tau_\nu(x, y, z)}\,\Delta l \qquad (1)$$

where j_ν is the emission per unit mass at the point (x, y, z) in the solar atmosphere, in the direction θ; ρ is the material density; Δl is the thickness of the radiating layer; and the exponent $-\tau_\nu$ is the fraction by which the energy emitted at the point (x, y, z) is diminished by absorption of the overlying layers. The quantity τ_ν is called the optical thickness of the material lying between the point (x, y, z) and the observer, and is defined

$$\tau_\nu = \Sigma\kappa_\nu\rho\Delta l \qquad (2)$$

where κ_ν is the mass absorption coefficient and the sum runs over all layers above (x, y, z). The summation in Eq. 1 extends from the observer downward into the sun until such a depth that $\exp(-\tau) = 0.1\epsilon$, in accordance with the definition of ϵ.

The roughest kind of knowledge of κ_ν shows that the thickness of the emitting layers on the sun must be small relative to the solar radius, so that we treat these layers as plane. Moreover, it is convenient to refer all distances to that measured along the solar radius at the point in question—that is, we replace Δl by $\Delta r \sec\theta$ and, in discussing optical thickness or optical depth, refer to that along the solar radius, which we denote by $\Delta\tau'$. Finally, we multiply and divide the summand in Eq. 1 by $\kappa_\nu\rho$ to reduce it to a more familiar form. Thus Eq. 1 becomes

$$I_\nu(\theta) = \Sigma\frac{j_\nu(\theta; x, y, z)}{\kappa_\nu(x, y, z)}e^{-\Delta\tau'\sec\theta}\,\Delta\tau'\sec\theta \qquad (3)$$

The sun is sufficiently close that we may observe the change in I_ν from the center to the limb of the solar disk—that is, as θ changes from 0° to 90°. Thus we have a possible two-parameter set of measures with which to work, I as function of ν and of θ. If we might assume explicitly what we have tacitly taken for granted—a statistical spherical symmetry for the sun—so that we might indeed talk of emission in layers, all functions of (x, y, z) become functions of r alone. And τ_ν' then depends on r and ν alone. We may rewrite Eq. 3 as follows:

$$I_\nu(\theta) = \Sigma\frac{j_\nu(\theta, \tau'_\nu)}{\kappa_\nu(\tau_\nu')}e^{-\tau_\nu'\sec\theta}\,\Delta\tau\nu'\sec\theta \qquad (4)$$

Thus the primary task confronting us in the use of Eq. 4 lies in the specification of the dependence of j_ν upon θ. For j_ν isotropic, we may replace j_ν/κ_ν by the single parameter $S_\nu(\tau_\nu')$, and determine $S_\nu(\tau_\nu')$ empirically, regarding Eq. 4 as a set of simultaneous algebraic equations, one equation for each θ. We call S_ν the source function. Either to inquire into the validity of the assumed isotropy, or to interpret the empirical $S_\nu(\tau_\nu')$, we require some comment on j_ν. We obtain this comment from two thermodynamic arguments: a consideration of the solar luminosity in its relation (i) to conditions in the region of energy generation and (ii) to the distribution of energy in the continuous spectrum.

Some Thermodynamic Considerations

The value 2.00 calories per second, per square centimeter for the solar constant leads to a value of 6.44×10^{10} ergs per second, per square centimeter for the energy flux at the solar surface, or a total emission of 3.92×10^{33} ergs per second from the sun. We call this figure the solar luminosity. If the sun were a thermodynamic enclosure, emitting as an ideal black-body, this energy flux would correspond to a temperature of 5800°K. We call this temperature parameter, which is based on the total quantity of solar radiation and which has no a priori connection with the spectral distribution, the effective temperature, $T_{eff.}$, of the sun. With these data, we may consider the thermodynamic arguments just stated.

Considerations on the region of energy generation. Up to the uncertainty introduced by the question of solar variability, the figure I have given for the solar luminosity also represents the rate of energy generation in the solar interior. One should be careful to note that even a true variability in solar emission does not necessarily imply a variability in energy production. It suffices to demonstrate the existence, somewhere in the sun outside the region of energy production, of a region that has large enough heat capacity to form the equivalent of a thermal valve and that has some tendency toward instability. This subject would itself form a separate topic for review, particularly in its application to the stars whose variability is well-established. I wish only to make the point that nothing in our knowledge thus far stands in essential contradiction to the notion of a steady rate of energy production in the sun over the afore-mentioned long geologic time scale.

In such a situation, we find that an appreciable energy production is possible only in the central regions of the sun. On the one hand, the large geologic time scale forces us to nuclear reactions for the solar energy source. Estimates of the conditions under which nuclear reactions in solar-type material could occur over the time scale required and with the total energetic output observed lead to a requirement of temperatures near (10^7) °K. On the other hand, the absence of observed change in the large-scale geometrical configuration of the sun suggests that the concept of a quiescent gas-mass, gravity balancing gas-pressure, is a good approximation to the actual state of affairs. The

roughest kinds of considerations, based only on the mass, radius, and rough chemical composition of the sun, independent of the observed luminosity, suffice to provide temperature estimates reaching the value (10^7) °K only for the central regions.

Under such conditions, we recognize that, apart from specifying the numerical value of the energy flux, the actual conditions in the region of energy generation have little bearing on our present interests, and the regions themselves are well-removed from those which are the immediate source of the observed emission. Indeed, recognizing that the energy density of radiation varies as T^4, and recalling the value of $T_{eff.}$, we see that the energy density in the regions of energy generation exceeds by a factor 10^{12} that in the surface regions. For all practical purposes, the interior of the sun is a thermodynamic enclosure. We expect an extremely low temperature gradient, corresponding to the diffusion of a relatively infinitesimal radiation flux outward. Under such conditions, the emission function j_ν is indeed isotropic; and the source function S_ν becomes the Planck function, $B_\nu\ (T)$.

With such a physical interpretation of S_ν, the results associated with a solution of Eq. 4 would become straightforward indeed, for our solution would give us $T\ (\tau_\nu)$ immediately from the directly determined $B_\nu\ (\tau_\nu)$. We might, however, expect possible difficulties in the region near the upper boundary of the sun, where the radiation flux becomes equal to the radiation density.

Distribution of energy in the continuous spectrum. On the basis of the discussion in the preceding section, we would expect to define a "local" temperature T at each point in the sun in terms of the local energy density of radiation. This temperature would specify the source function S_ν which enters Eq. 4 as the Planck function $B_\nu\ (T)$. However, while such a procedure seems reasonable when the energy density exceeds the energy flux, we raise the question of its validity when we approach the boundary where, by definition, the local energy density of radiation equals the radiation flux. We note that a literal application of these concepts developed for the interior calls for a boundary temperature T_0 near to and slightly less than $T_{eff.}$. Indeed, if we neglect to a first approximation the anisotropy of the radiation field, we see that $T_0 \sim T_{eff.} \times 2^{-\frac{1}{4}} \sim 4880°\text{K}$. For the radiation energy density varies as T^4; and in defining T_0 we have averaged the radiation over the whole sphere, while in $T_{eff.}$ we used only one hemisphere, thus introducing the factor 2.

Compare now the observed spectral distribution of energy with these two temperature parameters we have introduced, T_0 and $T_{eff.}$. Figure 1 contains

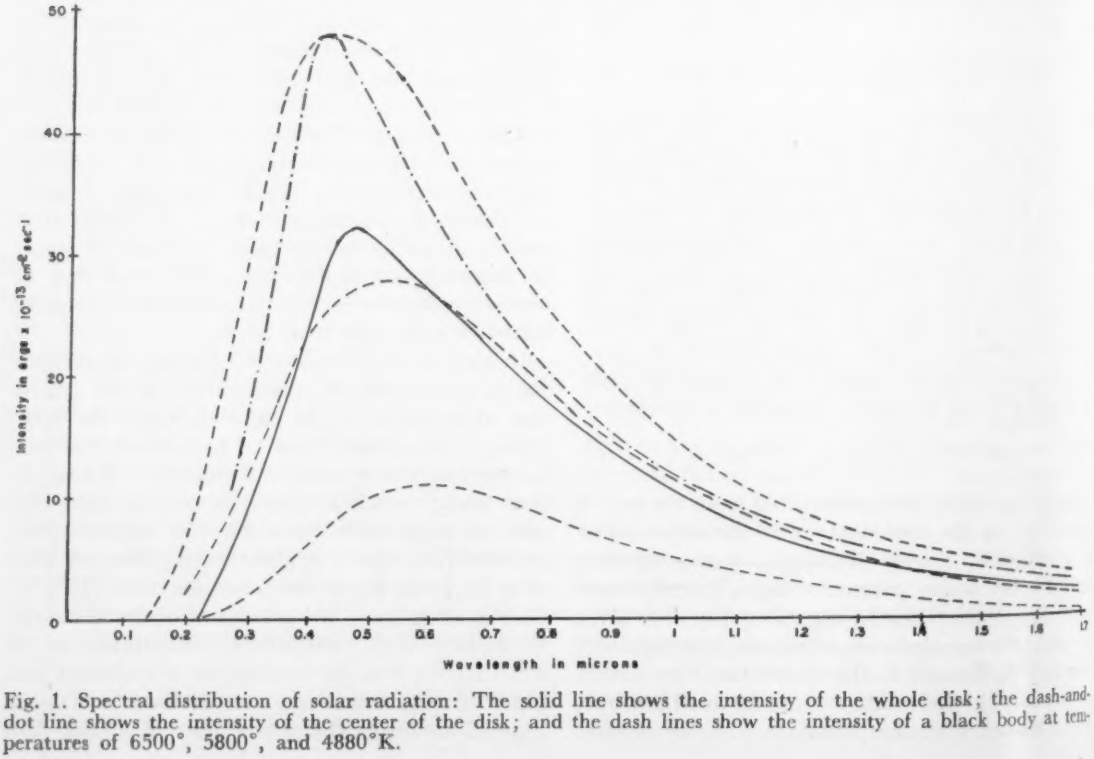

Fig. 1. Spectral distribution of solar radiation: The solid line shows the intensity of the whole disk; the dash-and-dot line shows the intensity of the center of the disk; and the dash lines show the intensity of a black body at temperatures of 6500°, 5800°, and 4880°K.

the observed data in the form of two curves, one for the radiation integrated over the whole solar disk normalized to unit solid angle, and one for the radiation from the center of the disk. The high-altitude rocket observations (*6*) on which the data for wavelengths shorter than 2900 angstroms are based exist only for the integrated radiation. The measures refer to values corrected to a position outside the terrestrial atmosphere. For comparison, three Planck curves are shown—for 5800°, 4880°, and 6500°K, the last corresponding to a Planck curve tangent to the point of greatest intensity of the observed curve from the center of the disk. We note several aspects and draw several conclusions.

First, the observed curve for the radiation integrated over the disk falls surprisingly close to the Planck curve for $T_{eff.}$, departing drastically only in the ultraviolet. Second, the Planck curve corresponding to the point of maximum intensity on the curve from the center of the disk corresponds to a temperature differing only by 700° from $T_{eff.}$. In addition, both observed curves lie between these two Planck curves, for 5800° and for 6500°K, over most of the normal optical spectrum. From these results, we conclude that adopting $B_\nu(T)$ for S_ν was a good approximation; that, except for local irregularities corresponding to absorption lines the absorption coefficient shows only a small variation with λ for 4000 A $< \lambda <$ 20,000 A; and that T varies relatively slowly with τ. By more detailed considerations, we can establish these conclusions considerably more convincingly than we have done here; but for our present purposes it suffices to remark that a significant departure in the solar atmosphere from any of these three conditions would cause a serious change in the energy spectrum produced.

Next, the observed curves for both center of disk and integrated light fall rapidly at wavelengths below 3500 angstroms, approaching a common value that lies near and below the Planck curve for T_o. From this result, we infer a rapid rise in the absorption coefficient near 3500 angstroms and a region of very small temperature gradient with $T \lesssim T_o$; for the curves corresponding to the disk center and to the integrated light differ by the effect of limbdarkening, the sec θ term in Eq. 4 which permits one to look only to a lesser radial depth, for the same optical path, at the limb. Thus, if the intensity from center and limb approach the same lesser value, we require a rise in absorption coefficient to reach the boundary regions, and permit only a slow temperature variation therein.

Finally, the result that the observed intensity in the ultraviolet falls below our computed T_o indicates two items of caution. First, we must be more precise in our approach to T_o from the energy density standpoint, including the limb darkening or nonisotropic effect in the computation. Second, we must be alert for the possibility that the rather simplified thermodynamic concepts we have adopted may break down—S_ν in this regime may possibly differ from B_ν.

Doubts and difficulties. Given the results of the preceding two sections, we have no difficulty in proceeding to construct a model of the solar atmosphere that is compatible with the observed data. Unfortunately, the situation resembles that summarized in our discussion of the solar constant. We should add the postscript "a model which satisfies the solar data—as far as they extend in the spectrum." And, as we see, the data do not extend down into the far ultraviolet. We do not have limb-darkening measures in the regions, which are thus far accessible only by rocket observation. Thus, we cannot by this procedure extend our empirical temperature distribution higher into the solar atmosphere, a region we find intriguing from the standpoint of T_o—its value, and how far we can extend the concept upon which it is based.

Now, again from the standpoint of direct information on that part of the solar spectrum of direct use in the terrestrial application of solar energy, the concern for these other parts of the spectrum is academic. The only spectral regions that can be of interest in the higher solar atmosphere are those having exceedingly large absorption coefficients, so that we do not see down into the atmospheric regions already discussed. But ostensibly such large absorption coefficients remove interest from the region by damming back energy that can be transported upward in this spectral region. Thus, we inferred the presence of a large absorption coefficient in the ultraviolet by the drop in the emission to a value corresponding to T_o or less. In essence, there can be no appreciable amount of energy in these upper-solar-atmospheric regions because the material there is transparent in the visible region, where the solar radiant energy lies concentrated, and because it is capable of absorbing energy only in those spectral regions that have low energy density.

Such was the logic of the thinking in most of our solar work until some 20 years ago. At this time the evidence became inescapable that something was seriously wrong with this straightforward picture. The outer solar atmosphere showed an extent and a supply of kinetic energy quite incompatible with the afore-mentioned notions of the energy supply it was capable of tapping. Over the next 10 years, the literature was full of last-ditch arguments, seeking ways of "beating" the energetic game sum-

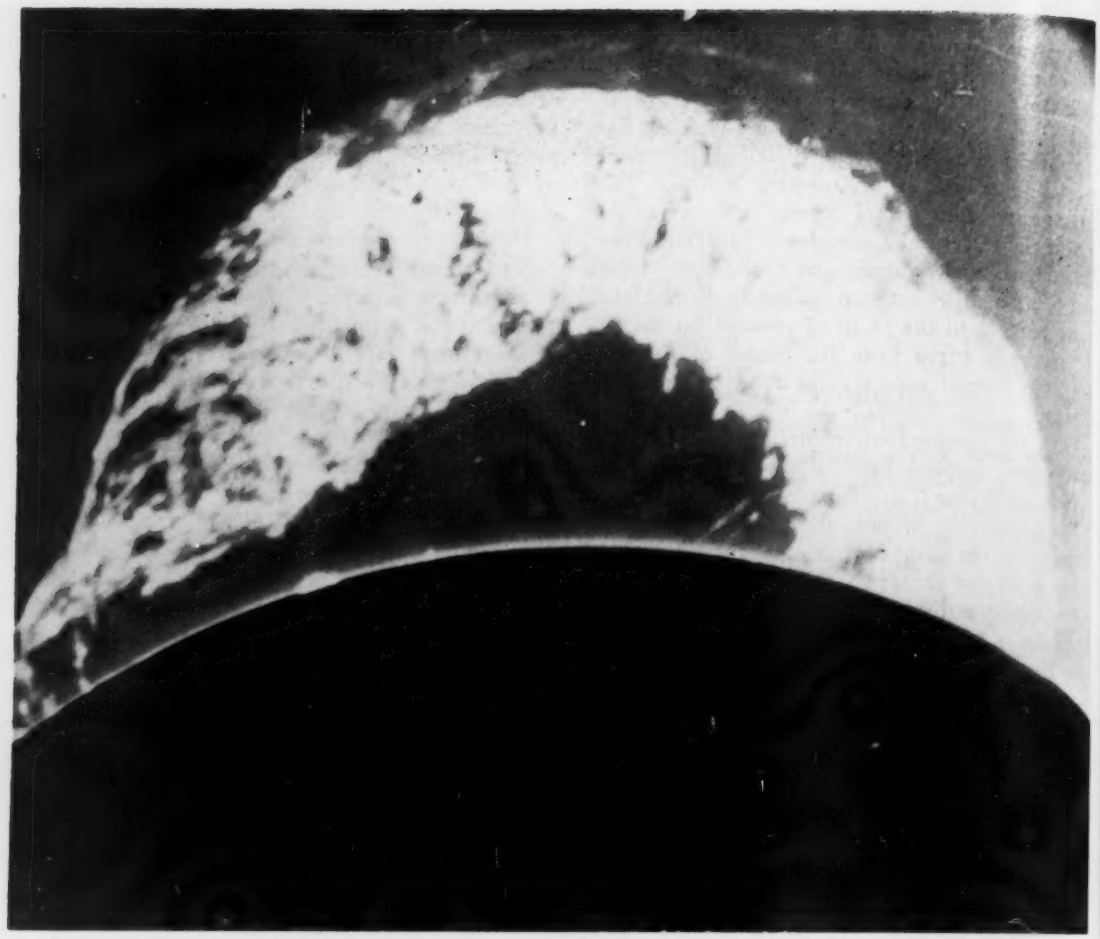

Fig. 2. Solar prominence of 4 June 1946. [Courtesy High Altitude Observatory]

marized here, yet retaining the conventional model of a solar atmosphere based on radiative transport of energy alone. Finally, in the last 10 years, the inhibitions have been cast aside, and the whole problem of the outer solar atmosphere and its anomalous energy supply has been examined from a wide range of viewpoints.

In essence, we conclude that the major energy supply must be mechanical rather than radiative. Some system having large kinetic energy feeds mechanical energy into the outer solar atmosphere, and thermal processes dissipate the supply, ultimately by radiation. As yet we have not identified this energy source, largely for two reasons. First, we have no clear idea how much energy we need to maintain this outer solar atmosphere. A solution to this problem depends on obtaining an empirical model of the thermal structure of the outer solar atmosphere, and thus a knowledge of the radiative dissipation. Second, we have no complete—or, indeed, even quasi-complete—idea of how much kinetic energy resides in the various systems of motions that we observe in the solar atmosphere. The well-known motion-picture records of prominence activity taken now at various solar observatories exhibit graphically one possibility. Figure 2 exhibits the largest eruptive prominence ever photographed. My own inclination leans toward a much smaller, yet more common system of motions called the spicules. Figure 3 is a typical photograph of a number of spicules on the solar limb. In any event, a study of all such systems of mechanical motion is required before we can provide a definitive answer to the problem.

The greatest progress to date has been made on specifying the thermal structure of the outer solar atmosphere. Although I have commented on the lack of limb-darkening observations in the region below 2900 angstroms, which would permit us to infer a temperature structure according to the methods summarized earlier, three other methods of approach have proved fruitful. First, observa-

tions at the time of total solar eclipse provide an approach akin to that of limb-darkening studies, and provide probably the most reliable data. An extension of this type of observation outside an eclipse just became feasible last summer with the completion of the 16-inch coronagraph at the Upper Air Research Observatory. However, the range of data is still not as great as that obtainable under good conditions at an eclipse. Second, observations at the centers of the stronger absorption lines provide data relating to these outer regions of the solar atmosphere. The greatest difficulty in the application of these methods—and the same problem is encountered in eclipse studies—lies in the almost certain failure of some of the concepts borrowed from studies of matter in a thermodynamic enclosure. Such change in concept introduces strong uncertainty in a specification of the temperature parameter, which under these nonequilibrium conditions is the Maxwellian kinetic temperature of the electrons. Third, the new science of radio astronomy provides a measure of the thermal properties of the highest regions in the solar atmosphere. Taken together, these three methods have in recent years provided us with the beginnings of a picture of the outer solar atmosphere.

Summary of Conditions in the Outer Solar Atmosphere

Photon component. In summarizing the results of our study of the ordinary optical spectrum, we stated that the situation resembled closely that in the study of the solar constant. We have a satisfactory set of observations sufficient to build a good working model of the structure of the solar layers which are primarily responsible for the emission in the ordinary optical region of the spectrum. However, when we turn to the radio-frequency and to the Lyman and x-ray regions, our observations are of very recent vintage; many of them are still indirect, such as inferences from the properties of the ionosphere on the intensity of the exciting solar radiation; and our knowledge of the environment wherein they originate is rudimentary. Nevertheless, by a few direct measures in these spectral regions of extremely long and extremely short wavelength and by a number of measures in the ordinary spectral regions of the radiation that comes from the outer solar atmosphere but is not ordinarily visible in direct disk photography, we have put together a rough picture of the outer solar atmosphere. This picture is best summarized, and joined with our picture of the lower solar atmosphere, by noting three possible divisions of the solar electromagnetic spectrum and commenting on their relationships.

First, we might divide the radiation according to a rough composite of wavelength regions, ease of measurement, and spectral regions of maximum energy. This is the division of the preceding paragraph and that discussed relative to the solar constant: the ordinary optical spectrum on the one hand, and the radio-frequency, Lyman, and x-ray regions on the other. Second, we divide the radiation spectrum according to whether it originates in the ordinary solar atmosphere or whether it originates in the outer solar atmosphere. Third, we might divide the radiation spectrum according to whether or not it shows a major variability. Then one can summarize most of the present results and thinking by the statement that all these three divisions are equivalent, either in characterizing the radiation field or the structure of the solar atmosphere.

In terms of the concepts already discussed, we view the outer solar atmosphere as a region produced by a system of mass motions, possibly akin to a large number of supersonic jets; this system dissipates its energy in an enveloping quiescent medium. Because the total mass in this medium is sufficiently small relative to the energy supply in the system of motions, very high kinetic temperatures can be produced, reaching, indeed, several million degrees. The optical opacity per atom in the very short and very long spectral regions is

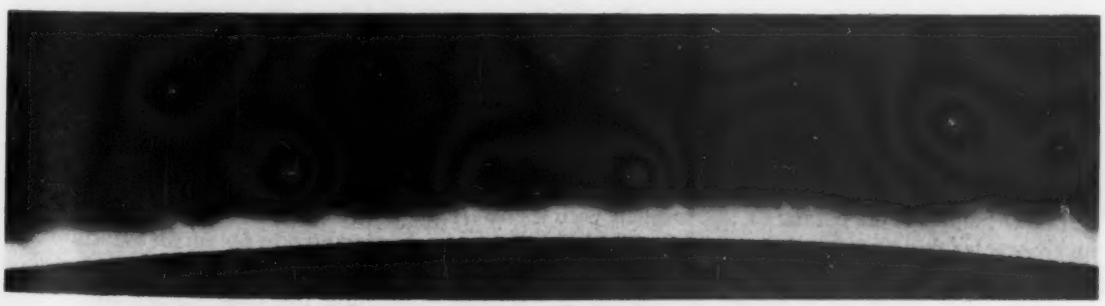

Fig. 3. Spicules on the solar limb. [Courtesy Air Force Upper Air Research Observatory]

high; hence an appreciable intensity of radiation in these spectral regions accumulates. The corresponding opacity in the ordinary optical regions is low; hence we observe little effect of the outer atmosphere in these spectral regions. (An exception occurs in the centers of the very strong Fraunhofer lines, where we see the higher atmospheric regions.) There is good evidence that the inferred structure of these outer solar regions varies with the cycle of solar activity. Thus, we must look for a corresponding change in the mechanical energy supply.

A major problem lies in the relative apportionment of mass between the system of mass motions proper and the enveloping atmosphere. This question, together with a more precise specification of all our present concepts, rests on the results obtained with some of our solar instruments that have just become operational.

Figure 4 illustrates the points just discussed, showing electron temperature as a function of height in the solar atmosphere. The portion of the curve at the left represents the temperature distribution in the photosphere, the source of the visual spectrum. The break in height scale represents our present uncertainty in connecting the several types of observations upon which the results for the different regions depend. In the chromosphere, the two curves represent an attempt to break down the structure of the outer solar atmosphere into two components, the "jet" structure which many of us believe represents the mechanical energy supply and the enveloping atmosphere. The two temperature curves refer to conditions in the two components. Rather tentatively, some of us have identified the cool component with the jet system and the hot component with the enveloping atmosphere.

As sketched in Fig. 4, the model for these upper atmospheric regions contains considerable uncertainty. On the one hand, it now appears virtually certain that the chromosphere, throughout the whole region shown, exhibits strong variability over the solar activity cycle. We do not presently have sufficient data to translate what we know about the variability into a variation in the thermal structure of Fig. 4. This particular diagram represents the results of a major effort to specify the properties of the chromosphere at one epoch, 1952, near the minimum of solar activity. We plan a similar effort at the eclipse of 1958, near the time of an expected maximum in solar activity. A combination of these results should permit us to comment with much greater certainty on the variability problem as it affects the temperature structure, and, in consequence, particularly as it relates to emission in the Lyman and x-ray spectrum.

There is another aspect in which Fig. 4 may be a misrepresentation of the actual situation in the solar atmosphere. In all our discussion—and, indeed, in virtually all the astronomical work to date—we have treated the photospheric regions as uniform. Now the gross irregularities of the solar disk due to the presence of sunspots is what has usually stimulated discussions of solar variability. In our analysis we have carefully avoided discussing the various terrestrial phenomena for which correlations have supposedly been found with sunspots, restricting our attention to solar features alone. We shall return to another aspect of solar variability connected with sunspots and the solar activity cycle in the next section. Here, I should simply like to call attention to another nonuniformity on the solar surface, the solar granulation, not so conspicuous as the sunspots because of the regularity of its nonuniformity and its apparent steady-state on a statistical basis.

The granulation makes its appearance as, and receives its name from, a mottling of the solar surface. Individual bright elements range downward in size from some 2500 kilometers as a maximum, with the average closer to 1000 kilometers, although some estimates place the average as low as 100 kilometers. The brightening of the element corresponds to a temperature rise over the surroundings; astronomers had conventionally placed the amount of the rise near several hundred degrees. Recently, suggestions have been made (7) that the temperature differences reach as high as 500° to 800°C. Such a situation would call for a serious change in our model of parts of the photosphere. The single curve in the lower regions of Fig. 4 may become a double curve. In such a situation, the effect on what we have called a satisfactory model of the solar region which produces the ordinary optical spectrum is presently the subject of much interest. It is not possible to dwell on this point here. The thinking is too undeveloped, most of the work having been done over the last 2 years. The situation is, however, indicative that all is not finally settled, even in the regions where we have apparently had the most success with our models.

As a final comment, one should mention that the granulation arises from a convective system of energy transport in the lower photosphere, and so we see the phenomenon as another example of what may happen when the purely radiative processes of energy transfer prove insufficient.

Corpuscular component. In the discussion thus far we have attempted to obtain an over-all picture of the solar energy spectrum, starting from those regions containing the most energy and working toward those containing the least. Our excuse for

a concern with the low-energy component at the Conference on Use of Solar Energy was the possibility of climatological and meteorological effects, and thus indirect influence on the solar energy which can be picked up at the bottom of the atmosphere. In this process, we discussed various aspects of solar variability, largely in conjunction with the configuration of the whole solar atmosphere or an average part of it. There is danger in such an approach. If there is one thing which the astronomers have learned over the last 20 years, in their exposure to problems of solar-terrestrial relationships, it is that solar events correlating with large terrestrial effect are localized, not general over the sun as a whole. A large magnetic storm in the terrestrial atmosphere may have as its solar correlation a single flare—or outburst of radiation in Hα—which occupies a relatively small area of the solar surface. Times of maximum solar activity are most likely to be associated with disturbed events in the terrestrial ionosphere, but the disturbance varies widely with the type of event and its geometry on the sun. Actually, the statement is slightly redundant in that we now think of the solar activity cycle rather than the sunspot cycle, with many parameters besides the sunspot number characterizing this activity, and with heavy weight being attached to solar parameters that are associated with terrestrial disturbances of one sort or another.

Although all these solar events are observed by their photon output, many terrestrial events suggest the presence of a corpuscular component which is at least as influential as the photon component in its terrestrial effect. Typical examples are the aurorae, the magnetic storms associated with flares, and the recently established detailed correlation between cosmic ray fluctuations and solar flares (*8*). Indeed, the very few well-established meteorological correlations would appear to be as much with parameters characterizing variability in the solar corpuscular radiation as with the photon component. As an example, I cite Shapiro's recent work (*9*) on the correlation between abrupt changes in the geomagnetic index, K_p, and the persistence correlation of the pressure pattern at the 500-millibar level. Shapiro found a tendency for a drop in the persistence correlation some 13 days after the abrupt change in K_p. Rob-

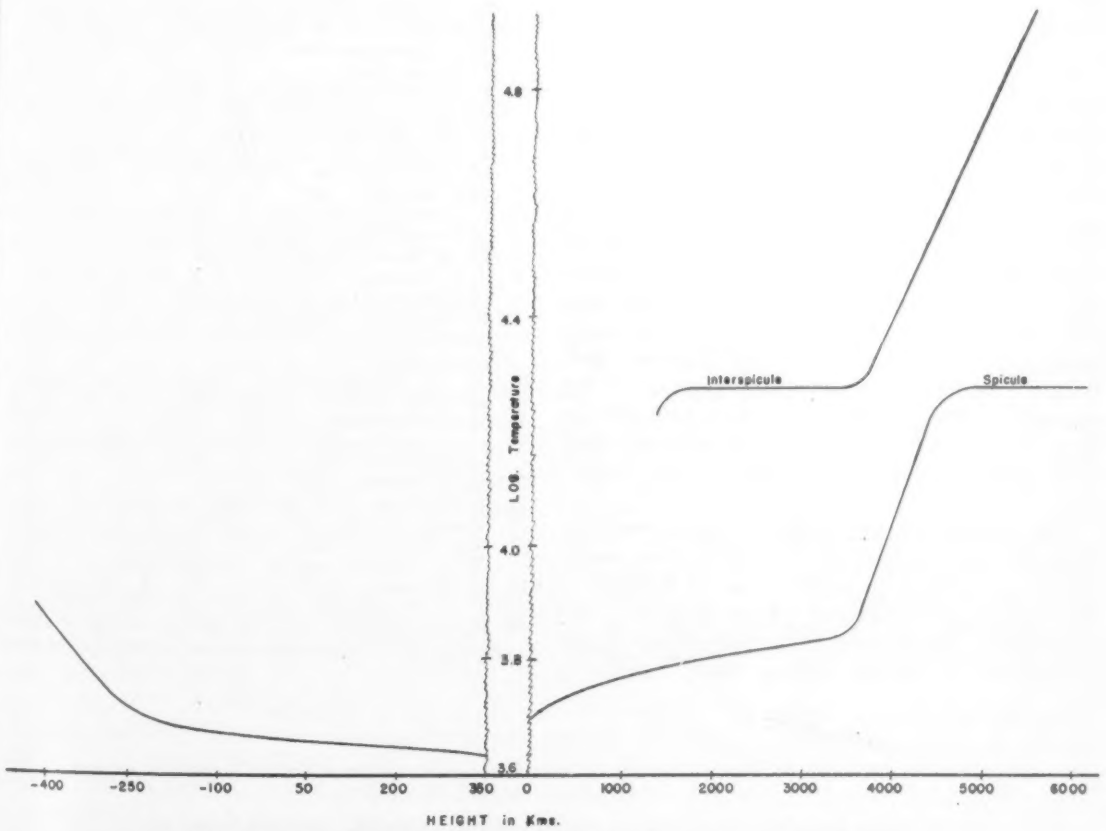

Fig. 4. Electron temperature as a function of height in the solar atmosphere.

erts, working with detailed observations from the coronagraph at Climax and Sacramento Peak, has attempted to relate the solar activity measures to these suggestions of Shapiro, and very preliminary results indicate a certain amount of success (*10*). Roberts has also attempted to discuss the relative importance and effect of the photon and corpuscular effect on the meteorological pattern. Specific conclusions depend on the particular geographical region. Roberts reports that recent work using fluctuations in the very short-wave photon component indicates correlations of a nature similar to the afore-mentioned work by Shapiro on the corpuscular component. Similar efforts of several years ago by Palmer (*11*) and more recently by Farthing (*12*) should be mentioned. It would appear that this direction of thinking is one of the most fruitful in solar-meteorological studies.

These remarks refer to fluctuations in the corpuscular component of solar radiation that is associated with certain solar events. The number of such solar events increases rapidly during the maxima of the solar-activity cycle—again a redundant remark. The occurrence of certain configurations of solar features provides an expectation pattern upon which we can make certain statistical predictions about terrestrial events. Thus, our physical understanding of the the phenomena is presently in a very undeveloped state. We can only be sure that under certain conditions the sun is able to produce a sufficiently energetic corpuscular flux to perturb the terrestrial atmosphere and to produce observable effects. Various theoretical investigations have been made of the problems involved in the approach of such a corpuscular cloud to the earth. Other analyses of possible accelerating mechanisms to eject the particles from the sun exist (*13*). In the main, such analyses refer to exceptional events, not to a steady-state situation. And, although we have approached this problem of the corpuscular emission from the data provided by these isolated events, we might ask whether there exists such a corpuscular component in the "quiet" sun.

The evidence on such a component is small indeed. Our only information is indirect. One source lies in a development of the ideas of Roberts on the relative meteorological effects of photon and corpuscular components. This possibility is largely unexplored. A second source, more extensively studied, lies in the investigation of the effect of solar corpuscular emission on the small bodies of the solar system—for example, from a study of cometary excitation on the one hand (*14*) and on the rate of disintegration of small meteoric material (*15*) on the other. Finally, a study of the low-energy cosmic-ray component (below 10 billion electron volts) provides an upper limit of the emission at these energies. Over-all, an estimate of a corpuscular flux of 10^6 ergs per second, per square centimeter seems a current reasonable value (*16*), although it may well be in error by a factor 100. We should recognize that all our uncertainties and difficulties of measure concerning the photon component are many times enhanced in the corpuscular component. It would appear, however, that rather extensive efforts to improve our knowledge in this area would be amply rewarded, at the very least in terms of the afore-mentioned tentative indications of solar-meteorological relationships.

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No more fiction for us: we calculate; but that we may calculate, we had to make fiction first.—NIETZSCHE.

Michael Faraday's Researches in Spiritualism

Although the last word has yet to be said about the various kinds of psychical phenomena under investigation today, some of the earlier issues have been settled and are heard about no more. As a historical excursion, we reprint on the following pages a short paper by Michael Faraday (1791–1867), which, in the subject matter studied, is quite unlike his better known investigations. The introductory remarks were prepared by the staff of The Scientific Monthly.

IN 1853, with the discoveries of electromagnetic induction, the laws of electrolysis, and the magnetic rotation of light behind him, Michael Faraday, with the aid of several skilled performers, conducted at a friend's house an investigation of certain spiritualistic phenomena then causing considerable stir. Using a few simple pieces of apparatus of his own design, he succeeded in establishing the same kinds of controls that characterized his more famous experimental researches. And he succeeded in providing an explanation for the observed set of phenomena that was satisfactory to all.

No new and strange forces were uncovered. Instead, Faraday demonstrated conclusively that the performers produced the observed effects by ordinary physical means. But here is what is noteworthy in this inquiry—and the reason Faraday's explanation proved to be satisfactory to both sides: he discovered that the performers were deceiving not just others but themselves as well. Although the performers produced the observed effects, they did so unconsciously.

Spiritualism in its modern phase began in March 1848 in Hydesville, New York, with the rappings and knockings of the Fox girls, and it spread rapidly to other parts of the United States, England, and the Continent. The set of phenomena broadened to include the tilting and levitation of tables and chairs, the playing of musical instruments without benefit of musicians, and the moving about of small objects in the dark. During the course of its development, several other leading figures in British physics also looked into the matter. Sir William Crookes, whose researches on the conduction of gases at low pressures preceded Sir Joseph Thomson's discovery of the electron, attended many of the seances of D. D. Home, a very skilled and personable medium, who, if he cheated, was never caught at it. Thomson, Sir Oliver Lodge, and Lord Rayleigh also attended seances. Crookes and Lodge were convinced by much of what they saw. Thomson and Rayleigh were not.

Faraday investigated two sets of phenomena, the levitation of tables and the turning of tables. In his "Observations on mental education," which was delivered as a lecture in 1854, he described the phenomenon of levitation as it was reported to occur.

"You hear at the present day, that some persons can place their fingers on a table, and then elevating their hands, the table will rise up and follow them; that the piece of furniture, though heavy, will ascend, and that their hands bear no weight, or are not drawn down to the wood; you do not hear of this as a conjuring manoeuvre, to be shown for your amusement; but are expected seriously to believe it, and are told that it is an important fact, a great discovery amongst the truths of nature" (*1*).

The second set of phenomena, the turning of tables, consisted in the performers placing their hands on the table, and then willing the table to turn in a certain direction or to move in a straight line. After the performers had waited a suitable period of time, the table was supposed to move of its own accord in the desired manner, dragging the hands of the performers along with it, or slipping under their hands.

Of his investigation of the second kind of phenomena, Faraday offered two accounts, a brief note and a more extended report. The note appeared 30 June 1853 in the *Times*. The report was published a few days later, 2 July, in the *Athenaeum*, a journal devoted to English and foreign literature, science, and the fine arts, which was founded in 1828 and which continues today, after several mergers, as the *New Statesman and Nation*. The investigation of levitation was not published, but Faraday assures us that his results here were also negative.

Although he was satisfied that the evidence supporting the claims of the performers was insufficient, Faraday undertook these investigations in order to be able to give a positive answer to the many letters he had received asking for his opinion. These letters, and the rapid and uncritical ac-

ceptance of spiritualistic forces, greatly disturbed Faraday because of what they revealed about the general level of intelligence. In a private communication to his close friend Christian Friedrich Schönbein, who discovered ozone, Faraday stated his innermost feelings on the subject, feelings which he was reluctant to admit, for he was both generous and pious.

"I have not been at work except in turning the tables upon the table-turners, nor should I have done that, but that so many inquiries poured in upon me, that I thought it better to stop the inpouring flood by letting all know at once what my views and thoughts were. What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a ridiculous world ours is, as far as concerns the mind of man. How full of inconsistencies, contradictions, and absurdities it is. I declare that, taking the average of many minds that have recently come before me (and apart from that spirit which God has placed in each), and accepting for a moment that average as a standard, I should far prefer the obedience, affections, and instinct of a dog before it. Do not whisper this, however, to others. There is One above who worketh in all things, and who governs even in the midst of that misrule to which the tendencies and powers of men are so easily perverted" (*2*).

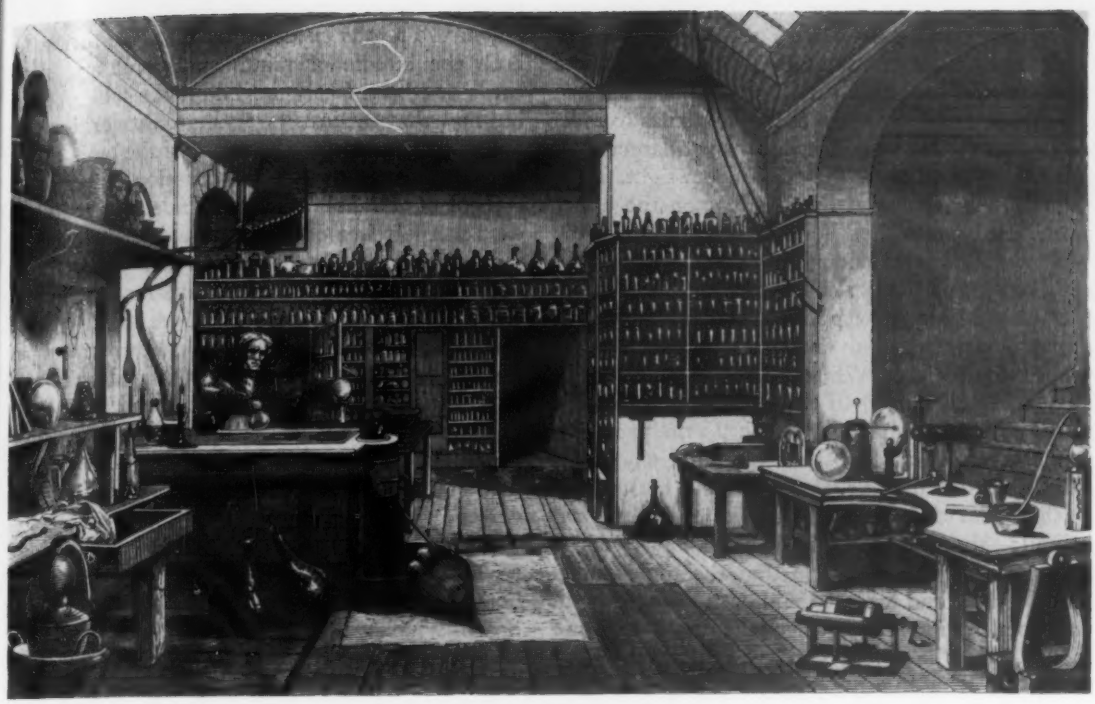
The appearance of about half a doezn works in 1853 in England and France on "la danse des tables", gives some measure of the interest then current in this set of phenomena. The claims of the table-movers reached the very issue of the *Athenaeum* in which Faraday's report was printed. On the page opposite the report, in a list of new publications, is a book titled *Table Turning and Table Talking*. An advertisement for this book, also in this issue, in part runs as follows: "Published this day, in ornamental cover, with numerous Illustrations, 1 *s.* 200 pages, TABLE TURNING and TABLE TALKING; containing detailed Reports of an infinite variety of Experiments performed in England, France, and Germany, with most marvellous results; also, minute Directions to enable everyone to practise them . . . ." Another work published that year bears the intriguing title, *Table-Turning, The Devil's Modern Masterpiece.*

We now reprint in full Faraday's report as it appeared in the *Athenaeum* (*3*). The report includes a brief excerpt from the earlier note in the *Times*.

Experimental Investigation of Table-Moving

The object which I had in view in this inquiry was not to satisfy myself, for my conclusion has been formed already on the evidence of those who had turned tables; but that I might be enabled to give a strong opinion, founded on facts, to the many

Expectant group gathered around a table is depicted in a pamphlet published in 1853 offering *Practical Instructions in Table-Moving*. The author, who does not give his name but describes himself as a physician, suggests that the tedium of waiting for results may be relieved if neighbors, whose fingers should touch, are of the opposite sex.



Laboratory at the Royal Institution, to which Faraday was appointed an assistant on 1 March 1813, at the recommendation of Sir Humphry Davy. Faraday became director and then Fullerton professor of chemistry, remaining in all 54 years.

who applied to me for it. Yet the proof which I sought for, and the method followed in the inquiry, were precisely of the same nature as those which I should adopt in any other physical investigation. The parties with whom I have worked were very honourable, very clear in their intentions, successful table-movers, very desirous of succeeding in establishing the existence of a peculiar power, thoroughly candid, and very effectual. It is with me a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power. They say, the table draws their hands; that it moves first, and they have to follow it,—that sometimes it even moves from under their hands. With some the table will move to the right or left according as they wish or will it,—with others the direction of the first motion is uncertain:—but all agree that the table moves the hands and not the hands the table. Though I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action, still I had no doubt of the influence of expectation upon their minds, and through that upon the success or failure of their efforts. The first point, therefore, was to remove all objections due to expectation, having relation to the substances which I might desire to use:—so, plates of the most different bodies, electrically speaking,—namely, sand-paper, millboard, glue, glass, moist clay, tinfoil, cardboard, gutta percha, vulcanized rubber, wood, etc.,—were made into a bundle and placed on a table under the hands of a turner. The table turned. Other bundles of other plates were submitted to different persons at other times,—and the tables turned. Henceforth, therefore, these substances may be used in the construction of apparatus. Neither during their use nor at other times could the slightest trace of electrical or magnetic effects be obtained. At the same trials it was readily ascertained that one person could produce the effect; and that the motion was not necessarily circular, but might be in a straight line. No form of experiment or mode of observation that I could devise gave me the slightest indication of any peculiar natural force. No attractions, or repulsions, or signs of tangential power, appeared,—nor anything which could be referred to other than the mere mechanical pressure exerted inadvertently by the turner. I therefore proceeded to analyse this pressure, or that part of it exerted in a horizontal direction:—doing so, in the first instance, unawares to the party. A soft cement, consisting of wax and turpentine, or wax and pomatum, was prepared. Four or five pieces of smooth slippery cardboard

were attached one over the other by little pellets of the cement, and the lower of these to a piece of sand-paper resting on the table; the edges of these sheets overlapped slightly, and on the under surface a pencil line was drawn over the laps so as to indicate position. The upper cardboard was larger than the rest, so as to cover the whole from sight. Then the table-turner placed the hands upon the upper card,—and we waited for the result. Now, the cement was strong enough to offer considerable resistance to mechanical motion, and also to retain the cards in any new position which they might acquire,—and yet weak enough to give way slowly to a continued force. When at last the tables, cards, and hands all moved to the left together, and so a true result was obtained, I took up the pack. On examination it was easy to see by the displacement of the parts of the line, that the hand had moved further than the table, and the latter had lagged behind;—that the hand, in fact, had pushed the upper card to the left, and that the under cards and the table had followed and been dragged by it. In other similar cases when the table had not moved, still the upper card was found to have moved, showing that the hand had carried it in the expected direction. It was evident, therefore, that the table had not drawn the hand and person round, nor had it moved simultaneously with the hand. The hand had left all things under it behind, and the table evidently tended continually to keep the hand back.

The next step was to arrange an index which should show whether the table moved first, or the hand moved before the table, or both moved or remained at rest together. At first this was done by placing an upright pin fixed on a leaden foot upon the table, and using that as the fulcrum of a light lever. The latter was made of a slip of foolscap paper, and the short arm, about ¼ of an inch in length, was attached to a pin proceeding from the edge of a slipping card placed on the table, and prepared to receive the hands of the table-turner. The other arm, of 11½ inches long, served for the index of motion. A coin laid on the table marked the normal position of the card and index. At first the slipping card was attached to the table by the soft cement, and the index was either screened from the turner, or the latter looked away: then, before the table moved, the index showed that the hand was giving a resultant pressure in the expected direction. The effect was never carried far enough to move the table, for the motion of the index corrected the judgment of the experimenter, who became aware that, inadvertently, a side force had been exerted. The card was not set free from the table, *i.e.* the cement was removed. This, of course, could not interfere with any of the results expected by the table-turner, for both the bundle of plates spoken of and single cards had been freely moved on the tables before; but now that the index was there, witnessing to the eye, and through it to the mind, of the table-turner, not the slightest tendency to motion either of the card or of the table occurred. Indeed, whether the card was left free or was attached to the table, all motion or tendency to motion was gone. In one particular case there was relative motion between the table and the hands: I believe that the hands moved in one direction; the table-turner was persuaded that the table moved from under the hand in the other direction:—a gauge, standing upon the floor, and pointing to the table, was therefore set up on that and some future occasions,—and then, neither motion of the hand nor of the table occurred.

A more perfect lever apparatus was then constructed in the following manner:—Two thin boards, 9½ inches by 7 inches, were provided; a board, 9 inches by 5 inches, was glued to the middle of the underside of one of these (to be called the table-board), so as to raise the edges free from the table; being placed on the table, near and parallel to its side, an upright pin was fixed close to the further edge of the board, at the middle, to serve as the fulcrum for the indicating lever. Then four glass rods, 7 inches long and ¼ in diameter, were placed as rollers on different parts of this table-board, and the upper board placed on them; the rods permitted any required amount of pressure on the boards, with a free motion of the upper on the lower to the right and left. At the part corresponding to the pin in the lower board, a piece was cut out of the upper board, and a pin attached there, which, being bent downwards, entered the hole in the end of the short arm of the index lever: this part of the lever was of cardboard; the indicating prolongation was a straight hay-stalk 15 inches long. In order to restrain the motion of the upper board on the lower, two vulcanized rubber rings were passed round both, at the parts not resting on the table: these, whilst they tied the boards together, acted also as springs,—and whilst they allowed the first feeblest tendency to motion to be seen by the index, exerted, before the upper board had moved a quarter of an inch, sufficient power in pulling the upper board back from either side, to resist a strong lateral action of the hand. All being thus arranged, except that the lever was away, the two boards were tied together with string, running parallel to the vulcanized rubber springs, so as to be immovable in relation to each other. They were then placed on the table, and a table-turner sat

down to them:—the table very shortly moved in due order, showing that the apparatus offered no impediment to the action. A like apparatus, with metal rollers, produced the same result under the hands of another person. The index was now put into its place and the string loosened, so that the springs should come into play. It was soon seen, with the party that could will the motion in either direction (from whom the index was purposely hidden), that the hands were gradually creeping up in the direction before agreed upon, though the party certainly thought they were pressing downwards only. When shown that it was so, they were truly surprised; but when they lifted up their hands and immediately saw the index return to its normal position, they were convinced. When they looked at the index and could see for themselves whether they were pressing truly downwards, or obliquely so as to produce a resultant in the right- or left-handed direction, then such an effect never took place. Several tried, for a long while together, and with the best will in the world; but no motion, right or left, of the table, or hand, or anything else occurred. [Beginning of the passage from the *Times*.]

The result was, that when the parties saw the index it remained very steady, when it was hidden from them, or they looked away from it, it wavered about, though they believed that they always pressed directly downwards; and, when the table did not move, there was still a resultant of hand force in the direction in which it was wished the table should move, which, however, was exercised quite unwittingly by the party operating. This resultant it is which, in the course of the waiting time, while the fingers and hands become stiff, numb, and insensible by continued pressure, grows up to an amount sufficient to move the table or the substances pressed upon. But the most valuable effect of this test-apparatus (which was afterwards made more perfect and independent of the table) is the corrective power it possesses over the mind of the table-turner. As soon as the index is placed before the most earnest, and they perceive—as in my presence they have always done—that it tells truly whether they are pressing downwards only or obliquely, then all effects of table-turning cease, even though the parties persevere, earnestly desiring motion, till they become weary and worn out. No prompting or checking of the hands is needed—the power is gone; and this only because the parties are made conscious of what they are really doing mechanically, and so are unable unwittingly to deceive themselves. I know that some may say that it is the cardboard next the fingers which moves first, and that it both drags the table, and also the table-turner with it. All I have to reply is, that the

This engraving of Faraday is a good likeness, according to James Clerk Maxwell. It is based on a photograph and appeared in volume 8 of *Nature* as the first of a series of portraits of great scientists.

cardboard may in practice be reduced to a thin sheet of paper weighing only a few grains, or to a piece of goldbeaters' skin, or even the end of the lever, and (in principle) to the very cuticle of the finger itself. Then the results that follow are too absurd to be admitted: the table becomes an incumbrance, and a person holding out the fingers in the air, either naked or tipped with goldbeaters' skin or cardboard, ought to be drawn about the room, etc.; but I refrain from considering imaginary yet consequent results which have nothing philosophical or real in them. [End of the passage from the *Times*.]

Another form of index was applied thus:—a circular hole was cut in the middle of the upper board, and a piece of cartridge paper pasted under it on the lower surface of the board; a thin slice of cork was fixed on the upper surface of the lower board corresponding to the cartridge paper; the interval between them might be a quarter of an inch or less. A needle was fixed into the end of one of the index hay-stalks, and when all was in place the needle point was passed through the cartridge paper and pressed slightly into the cork beneath, so as to stand upright: then any motion of the hand, or hand-board, was instantly rendered evident by the deflection of the perpendicular hay-stalk to the right or left.

I think the apparatus I have described may be

useful to many who really wish to know the truth of nature, and would prefer that truth to a mistaken conclusion; desired, perhaps only because it seems to be new or strange. Persons do not know how difficult it is to press directly downward, or in any given direction against a fixed obstacle, or even to *know only* whether they are doing so or not; unless they have some indicator, which, by visible motion or otherwise, shall instruct them: and this is more especially the case when the muscles of the fingers and hand have been cramped and rendered either tingling, or insensible, or cold by long-continued pressure. If a finger be pressed constantly into the corner of a window-frame for ten minutes or more, and then, continuing the pressure, the mind be directed to judge whether the force at a given moment is all horizontal, or all downward, or how much is in one direction and how much in the other, it will find great difficulty in deciding; and will at last become altogether uncertain: at least such is my case. I know that a similar result occurs with others; for I have had two boards arranged, separated, not by rollers, but by plugs of vulcanized rubber, and with the vertical index: when a person with his hands on the upper board is requested to press only downwards, and the index is hidden from his sight, it moves to the right, to the left, to him and from him, and in all horizontal directions; so utterly unable is he strictly to fulfill his intention without a visible and correcting indicator. Now, such is the use of the instrument with the horizontal index and rollers: the mind is instructed, and the involuntary or *quasi* involuntary motion is checked in the commencement, and therefore never rises up to the degree needful to move the table, or even permanently the index itself. No one can suppose that looking at the index can in any way interfere with the transfer of electricity or any other power from the hand to the board under it or to the table. If the board tends to move, it may do so, the index does not confine it; and if the table tends to move, there is no reason why it should not. If both were influenced by any power to move together, they may do so, as they did indeed when the apparatus was tied, and the mind and muscles left unwatched and unchecked.

I must bring this long description to a close. I am a little ashamed of it, for I think, in the present age, and in this part of the world, it ought not to have been required. Nevertheless, I hope it may be useful. There are many whom I do not expect to convince; but I may be allowed to say that I cannot undertake to answer such objections as may be made. I state my own convictions as an experimental philosopher, and find it no more necessary to enter into controversy on this point than on any other in science, as the nature of matter, or inertia, or the magnetization of light, on which I may differ from others. The world will decide sooner or later in all such cases, and I have no doubt very soon and correctly in the present instance. Those who may wish to see the particular construction of the test apparatus which I have employed, may have the opportunity at Mr. Newman's, 122 Regent Street. Further, I may say, I have sought earnestly for cases of lifting by attraction, and indications of attraction in any form, but have gained no traces of such effects. Finally, I beg to direct attention to the discourse delivered by Dr. Carpenter at the Royal Institution on the 12th of March, 1852, entitled, "On the Influence of Suggestion in modifying and directing Muscular Movement, independently of Volition;" which, especially in the latter part, should be considered in reference to table-moving by all who are interested in the subject.

M. FARADAY.

Royal Institution, June 27

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An engineer is a man who knows a great deal about a very little, and who goes along knowing more and more about less, until finally he knows practically everything about nothing. A salesman, on the other hand, is a man who knows a very little about a great deal, and keeps knowing less and less about more, until finally he knows practically nothing about everything—UNION OIL BULLETIN.

Historical and Thematic Relations of Psychology to Other Sciences

EGON BRUNSWIK

The author received his Ph.D. at the University of Vienna in 1927 and taught at the same institution for a number of years. At the time of his death on 7 July 1955, at the age of 52, he was professor of psychology at the University of California in Berkeley and a member of the board of trustees of the Institute for the Unity of Science in Boston, Mass. Among his publications related to the subject matter of this paper was The Conceptual Framework of Psychology. *This article is based on a paper presented before the Section on History and Philosophy of Science at the Berkeley meeting of the AAAS in December 1954.*

NOT quite a century has passed since experimental psychology began, in Gustav Theodor Fechner's treatise on the "psychophysics" of sensation in 1860, to emancipate itself as a science. The emancipation has taken place relative to the purely speculative approach of philosophy, on the one hand, and relative to the confinement to the human or animal body imposed within psychology's closest antecedent among the sciences, physiology, on the other. And not quite half of a century has elapsed since John B. Watson, in 1913, suggested that psychology abandon its original subjectivistic or introspectionistic concern with sensation and other data of consciousness and concentrate on the "behavior" of the organism as a physical body in a physical environment. Thus psychology was to be placed fully under the auspices of the methodologically most rigorous of its older sister disciplines, physics.

In the light of a comparative science, psychology stands at the crossroads as perhaps none of the other disciplines does. I shall stress especially its relationships to the physical and biological sciences, including some of the relatively "low-brow" cultural disciplines such as economics.

From Physiology to Physiological Psychology

The emergence of what we may call the specific "thema" of psychology is best discussed by contrasting the physiological psychology of today with the physiology from which it has sprung. Some of the major physiological discoveries of the first half of the 19th century were more or less directly at the doorstep of psychology. Among these were the Bell-Magendie law, which asserts the structural and functional discreteness of the sensory and motor nerves, and the law of specific sense or nerve energies by Charles Bell, Johannes Müller, and Helmholtz, which recognizes the dependence of sensation on the receiving organism. Still another discovery, the establishment of the rate of nervous impulse by Müller and by Helmholtz, best represents the step-by-step tracing of internal processes which is so characteristic of physiology; this is symbolized by the straight line in diagram *A* of Table 1.

Compare this pattern with the counterpart of rate of nervous impulse in psychology proper, reaction time. The distinguishing characteristic of problems of this latter kind is the concentration on the over-all functional correlation of sensory input and motor output without primary concern for the details of the mediating process. This correlational peripheralism is described by the bridgelike arc in schema *B* of Table 1. In line with its gross, achievement-oriented character, the study of reaction time received its first impetus from difficulties with observational error in astronomy raised by Bessel in the 1820's; later it became a favorite of Wilhelm Wundt, the founder of the first psychological laboratory at Leipzig in 1879, and of his American assistant, James McKeen Cattell, who applied it to his differential-psychological testing research at Columbia University.

The direct physical observability of both stimulus and response renders the study of reaction time a 19th century rudimentary anticipation of Watson's sensory-motor behaviorism and of Bekhterev's concurrent reflexology. More importantly, Fechner's psychophysics shared with the study of reaction time a relational rather than a process-centered emphasis. This was manifested in the famous Weber-Fechner law which expresses sensation as a direct mathematical function of the external stimulus. The fact that psychophysics is

being considered almost unanimously the birth cry of psychology proper must be ascribed to this correlational feature.

Various conditioning and higher learning problems have recently been treated under the sensory-motor reflex schema by Skinner and others. Critics have bemoaned the fact that this approach, cutting short as it does from input to output, tends to bypass the brain; and the dean of historians of psychology, Edwin G. Boring, has criticized it as a "psychology of the empty organism" (*1*).

In the development of physiological psychology, the possibility of such an accusation is circumvented by the emergence of a third type of approach that at the same time does away with peripheralism in its various forms. It is described in Table 1 under diagram *C*. Occurrences in the brain—that is, "central" factors—are directly correlated with relatively remote, or "distal," results of behavior ("achievements"), such as the reaching of the end of mazes of varying intricacy by a rat (right arc; the arc to the left is shown to indicate that abstraction and related cognitive extrapolations into the causal ancestry of the stimulus impact are inseparably intertwined with all brain-and-achievement studies).

Foreshadowed by Gall's notorious "phrenology" in the early 19th century and by Flourens' pioneering of brain extirpation experiments soon thereafter, the central-distal approach reached its full scope in the brain-lesion study in rats by Lashley in 1929 (*2*). The same year brought Berger's report on brain waves and thus the beginning of electroencephalography with its wide use in modern psychiatry. More recently, Halstead has applied the statistical tool of factor analysis to the study of brain and intelligence at the human level (*3*).

In contrast, the 1860's and 1870's witnessed the peripheralistically conceived brain-localization studies of Broca and of Fritsch and Hitzig in which the more narrowly sensory or motor aspects were stressed at the expense of organization and integration.

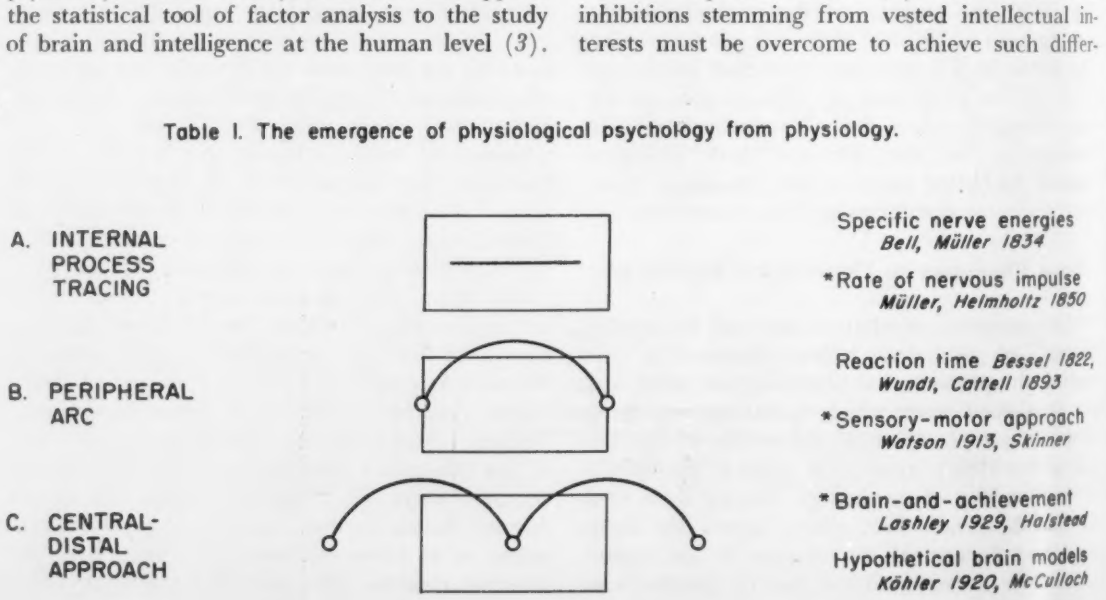
Of considerably shorter history than the empirical brain-and-achievement studies are the largely hypothetical studies of the brain which began with Köhler's theory of dynamic brain fields or "physical Gestalten" in 1920 (*4*). While the Gestalt-psychological approach is more purely central rather than genuinely central-distal, the distal adjustmental aspects have come to share the limelight in the study of "teleological mechanisms" by McCulloch and other cyberneticists (*5*).

Psychology and the Ancient Speculative Unity of Science

One of the most prominent problems of a comparative science of science is that of the unity of the sciences (*6*). Most scientists agree that there must be unity with respect to the objectivity of both observation and the procedural aspects of theory construction. Physiological psychology and the school of behaviorism are primarily dedicated to the unification of psychology with the natural sciences along these lines. Equally important as the procedural unification is the thematic diversification of the sciences, however. I have therefore made it a point to begin these considerations with an example of such diversification of psychology from a neighboring discipline.

Close inspection shows (*6*) that considerable inhibitions stemming from vested intellectual interests must be overcome to achieve such differ-

Table I. The emergence of physiological psychology from physiology.



entiation among the sciences. We must be on guard against excessive thematic unity, especially if we are concerned with a younger discipline growing up in the shadow of overwhelming parent sciences.

Formidable and even grotesque examples of an excessive unity of a highly uncritical kind can be brought forth from ancient science. More or less arbitrary "dichotomies" or other formalistic classification schemes are combined to pervade an all-encompassing system. An example involving the psychology both of sensation and of personality along with physics and physiology is presented in Table 2. For the most part, the schema is based on the pre-Socratic cosmology of Empedocles and on the humoral doctrine of four temperaments of Hippocrates and Galen; the last two columns are relatively modern elaborations (*7*). The original dichotomies are developed into quadripartite systems either by doubling or by compounding so that a modicum of differentiation is achieved.

From the systematic point of view, two features must be especially emphasized in connection with Table 2. One is the arbitrariness of classification as revealed most drastically by the presence of alternative sets of columns for the same subject matter—for example a double dichotomy and a partly conflicting compound dichotomy for the sensory qualities. (We may add that another of the pre-Socratics, Anaximander, chose air to be cold rather than dry.)

The other feature noteworthy in Table 2 is the apparent ease of transfer of four-ness from one area to another in the manner of an absolute one-to-one correspondence. Different areas of knowledge, capable of independent approach, are thus thrown together indiscriminately by means of vague analogy; this is comparable to what such child psychologists as Piaget or Heinz Werner have described as syncretic or diffuse modes of thought (*8*).

More specifically, Gestalt psychologists have criticized the ready assumption of a strict correspondence between physical stimuli and sensory qualities as an undue "constancy hypothesis." It is in this surreptitious manner that physics and sensory psychology (Table 2, columns *a* to *c*) become symmetrical and thus in effect merge into one. It is even difficult to reconstruct which of the two areas of knowledge has the observational primacy over the other, although it is evident that there is a good deal of give and take.

In philosophy, it is easily seen that the implied operational indistinguishability of matter and mind (in this case, sensation) constitutes, or at least reinforces, naive realism; or else, by way of the horizontal dichotomy between columns, it helps to put dualism on an absolute basis. Once the constancy hypothesis of the coordination of the two realms has given rise to the accusation of "unnecessary duplication" (as in Occam's razor), this dualism in turn readily changes into either materialistic or idealistic monism. The regularity and symmetry which result from easy transfer and carry with them the flavor of Pythagorean number mystics may be criticized on the same grounds of subjectivism on which Schopenhauer criticized Kant's compulsive filling of all the plots in his 3×4 table of categories.

(In experimental psychology, the adoption of the constancy hypothesis in its radical form would lead to the obliteration of the stimulus-response problem of psychophysics which, as we have seen, lies at the roots of modern psychology; it would even lead to the at least theoretical impossibility of acknowledging any kind of illusion—as it has come close to doing in Locke's doctrine of primary qualities, such as size, shape or motion.)

Both the arbitrariness and the easy transfer that characterize early stages of science are further revealed in the fact that some systems are not dichotomous or fourfold but three-, five-, or sevenfold. In his capacity as a psychologist, Plato distinguished three major faculties (reason, emotion, and desire, the latter including the lowly sensation); he localized them in a corresponding hierarchy of physiological centers (brain, heart, and liver or "phren"—that is, diaphragm); and he further distinguished three corresponding sociopolitical personality types (philosopher, warrior, and worker). The ancient Chinese favored a fivefold scheme. In the doctrine of cosmic elements, the air of the Greeks is replaced by metal, and wood is added as a fifth element; the scheme is syncretically generalized to five tastes, five intestines, five sentiments, five poisons, five planets, five dynasties, and so forth (*9*). The relative merits of the various base numbers are not discussed here, although it may be granted that some of them are not without a realistic basis in certain limited areas (such as two-ness for sex, three-ness for man between input and output or the healthy medium between extremes, and so forth).

Relative Level of Maturity of Psychology

As has been noted in passing, the most distinctly psychological aspects of the doctrine of four temperaments have outlasted their counterparts in physics and physiology by centuries if not millenniums. Furthermore, this doctrine has flourished in much greater variety and thus is fraught with more ambiguity than its long-vanished corre-

spondents in the natural sciences. Columns *g* and *h* of Table 2 show only two of the kinds of compound dichotomies usually suggested, both conceived in the Wundtian three-dimensional theory of emotion; Herbart used a combination of strong-weak and pleasant-unpleasant instead. There are at least 16 major thinkers who expended their efforts on the four temperaments in a feast of arbitrary classification. Among the persons concerned were Kant and such serious experimental psychologists of the past as Ribot, Külpe, Ebbinghaus, Höffding, and Meumann; on the contemporary scene we find the well-known German typologist, Ludwig Klages (*7*). This suggests that the relative youth of psychology is matched, at least in the personality area concerned, by a backwardness in its categorial structure, or "modes of thought."

In investigating the question of the relative maturity of psychology further, we note that dichotomizing and related forms of absolute classification, as well as their formalistic-syncretic transfer to other areas, are but two of several aspects of a broader prescientific syndrome. Auguste Comte put his finger on this syndrome in his distinction between what he called the metaphysical and the positive stages of science; with an eye on the special situation in psychology, Kurt Lewin, somewhat similarly, distinguished between Aristotelian and Galileian modes of thought (*10*). According to Lewin, progress from the former to the latter mode involves any or all of the following, partly overlapping shifts: from dichotomies to gradations, from qualitative appearance to quantitative reality, from subjective speculation to objectivism, from classification to causation, from phenotype to genotype, from static existence to dynamic flow, from surface to depth, and from disjointed description to the "nomothetic" search for laws.

We may try to assess the standing of psychology among the sciences by listing a few of the most crucial shifts in these respects (Table 3). Perhaps the earliest shift from phenotypical quality to genotypical quantity concerns physics. From Empedocles' qualitatively conceived fourfold scheme mentioned in a previous paragraph, the doctrine of elements moved on toward an essentially modern conception of physical reality in Democritus' atomic theory that stressed shape and size instead of sensation. This theory is far from free of subjective speculation or contamination by direct perceptual appearances (especially "synesthesia" from the tactile-kinesthetic sphere), to be sure, but the step from surface to underlying reality and

Table 2. Simple and compound dichotomies and a resultant pervasive system of corresponding quadripartite schemes in ancient physics and physiology and in personality psychology.

PHYSICS	SENSORY PSYCHOLOGY		PHYSIOLOGY	PERSONALITY PSYCHOLOGY			
	Dichotomies of qualities (Alternatives:)			Temperaments and their behavioral aspects		Compound dichotomies (Alternatives:)	
Cosmic elements	Double	Compound	Humors			Emotional response	Affective tone
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Air	Dry	Warm-Moist	Blood	Sanguine	Hopeful	Weak-Quick	Pleasant-Excited
Earth	Cold	Cold-Dry	Black bile (Spleen)	Melancholic	Sad	Strong-Slow	Unpleasant-Calm
Fire	Warm	Warm-Dry	Yellow bile	Choleric	Irascible	Strong-Quick	Unpleasant-Excited
Water	Moist	Cold Moist	Phlegm (Mucus)	Phlegmatic	Apathetic	Weak-Slow	Pleasant-Calm

from dichotomy to gradation is taken at least in intent. The step from perceptual appearances to an indirect, abstract construction of a much more dynamically conceived reality was next made in astronomy with the shift from the perceptually dominated geocentric to the nomothetically more economical Copernican system.

The biological sciences followed with the shift from static anatomy to dynamic physiology as epitomized by Harvey's discovery of the circulation of the blood, and with the shift from Linnaeus' phenotypical taxonomy to Darwin's genotypical evolutionary classification in botany and zoology. Transitions between dichotomizing and gradations also occurred—for example, when in the Middle Ages the four humors were ranked according to their "degree" of aliveness (*11*).

Confirming our suspicion, we note that corresponding steps in the psychological disciplines follow much later, mostly within the memory of ourselves or of our immediate elders. In psychiatry, there is a tradition of static description and cataloging which began in the early 17th century with Robert Burton's revealingly titled *Anatomy of Melancholy*, which continued with Pinel—the man who freed the insane from prison during the French revolution—and was still in evidence in Kraepelin until it was broken by Freud's "depth-psychological" revision of psychiatric classification, notably in the doctrine of neurosis. In psychology proper, there is the shift from Wundt's and Titchener's so-called "existential" inventory and description of sensory experiences to Lewin's more dispositionally conceived notions of the internal psychological "field." Instead of Lewin, I might have mentioned some of his older Gestalt-phychological colleagues, notably Wertheimer and Köhler. Beginning in the 1910's, these workers set out to work on the intrinsic central dynamics of perception, and of thinking and problem solving; by virtue of their introspectionistic orientation, they are more comparable to Wundt and Titchener than to the more behaviorally oriented Lewin. Indeed, the simile has sometimes been used that while Titchener tried to dissect consciousness analytically like an anatomist, and his "sensations" thus are no better than a carcass of experience, the gestaltists with their "phenomenology" are more like physiologists in that they keep consciousness alive while studying it.

As in all structural interpretations of history, a table of examples can be no substitute for full documentation. Indeed, Hippocrates' humoral underpinning of the doctrine of temperaments may be set parallel to Democritus' geometric underpinning of the elements and offered as demonstration of the fact that at least part of psychology showed objectivistic intent as early as did physics. Yet humoral doctrine is physiology, not psychology; nor would the fact that much of ancient psychology was behavioristic from the outset change our impression that, in the handling of the actual problems in the area, relatively primitive patterns of thought were the rule. I have already mentioned in discussing Table 2 that syncretic dichotomizing persists much longer and flourishes more abundantly in the psychological doctrine of the four temperaments than it does in the corresponding doctrines of the four physical elements or of the four physiological humors. We may further remind ourselves of the fact that in the social sciences—in many ways still younger than psychology—elaborate dichotomous schemes are still in vogue in some quarters right under our eyes—for example, in the work of Talcott Parsons.

Dependence of Psychology on the Natural Sciences

Next we turn to more direct cross-disciplinary comparisons that involve historical phase differences with respect to comparable categories and in which psychology appears at the receiving end. For chronologically arranged evidence, we may turn to Table 4. This table concentrates on the experimental and differential-psychological developments that constitute the core of modern psychology; developments in physiological and abnormal psychology which are incorporated in some of the preceding tables have been played down or omitted. Special emphasis is given to conceptual outlook and methodology.

Our first consideration concerns the law-stating or nomothetic approach; it is traced at the left side of the table. While the actual establishment of natural law as it has been able to stand the test of time was brought about in astronomy and physics during the 17th century, psychology had to wait until Fechner (1860) for the beginnings of the experimental-nomothetic treatment of sensation, and until Egginghaus (1885) for that of memory by association. Solid arrows indicating these cross-disciplinary infusions generally point downward in a telltale manner in the respective parts of the table. Thus axiomatization, or more generally what Feigl called higher-order theory (*12*), was brought about in physics by Newton; much less impressive attempts in psychology—further preceded and indeed prompted by Hilbert's axiomatization of geometry and by Woodger's efforts toward an axiomatization of biology (as his contribution to the *International Encyclo-*

pedia of Unified Science)— had to wait until the work of Hull and his associates in the 1940's (*13*). A perusal of the writings of such nomothetically oriented psychologists as Hull or Lewin reveals that the ostensible classic among physical laws, the law of falling bodies, is invoked as an exemplar with almost monotonous regularity.

The nomothetic ideal is paramount not only in the classical phase of experimental psychology in the 19th century and in the recent postulational behaviorism of Hull but also in Gestalt psychology and in the physiological theory of Gestalt referred to earlier in this article. Frequent reference to "dynamics" in a brain "field" suggests analogies to Maxwell's electromagnetic field theory. Warnings, pointing out that gravitation also acts in a field, have been sounded against pressing this analogy; a broken, rather than solid, arrow has therefore been used in Table 4. The fact remains, however, that the revolutionary element in Gestalt psychology is the breaking away from elementism and associationism and that the "machinelike" models to which these conceptions can be traced largely originated in classical mechanics. In addition, the coexistence of associationistic and of field-dynamic principles in modern psychological theory in certain ways resembles the duality of gravitation and electromagnetism of which modern physics has so long been tolerant.

The 20-odd years about the turn of the century were a particularly turbulent phase in the development of psychology—so much so that Karl Bühler has spoken of them as the constructive crisis of psychology (*14*). Gestalt psychology and classical behaviorism are but two especially clear-cut of at least four new psychological movements that sprang up in that period.

Another of these new movements is psychoanalysis. It shares with Gestalt psychology the insistence on the finding of regularities of a more complex scope. Freud, who had a distinguished active career in physiology prior to developing his dynamic theory of personality was, as was documented by Siegfried Bernfeld (*15*), strongly influenced by the physical thinking of his time. He was strongly influenced by Helmholtz' principle of conservation of energy in developing his basic models. As was further demonstrated by Else Frenkel-Brunswik at the joint symposium of the AAAS with the Institute for the Unity of Science in Boston in 1954 (*16*), Freud had a keen sense of the basic requirements of the philosophy of science, popular belief to the contrary notwithstanding.

Table 3. Shift in modes of classification and outlook from the subjective-qualitative-phenotypical-static ("Aristotelian") to the objective-quantitative-genotypical-dynamic ("Galileian") syndrome.

Physics (Elements)	Astronomy	Anatomy-Physiology	Biology	Psychiatry	Psychology
Empedocles ↓ *5th B.C.*					
Democritus *4th B.C.*	*Ptolemy* ↓ *2d A.D.*				
	Copernicus *1530*	*Vesalius 1543* ↓ *Harvey 1628*			
			Linnaeus ↓ *1738*		
			Darwin *1859*	*Kraepelin* *1883* ↓ *Freud 1900*	
					Titchener 1901 ↓ *Lewin 1935*

In Watson's classical behaviorism, we note that its primary concern was the fulfillment of the physicalistic ideal of observational precision. In the American psychology of this period, the desire for fact-finding and the fear of the dangers of speculation temporarily took precedence over the nomothetic aim and led to a form of descriptive empiricism or factualism that had the earmarks of Bacon's "simple enumeration" or of the early antitheoretical positivism of Comte or Mach. In stressing fact more than law, the prime urgency of the "general" was challenged in favor of the "particular," and thus a first inroad was made on the nomothetic ideal of science insofar as it concerns psychology. As I will try to show in the next section, psychology seems to drift toward a course halfway between factualism and nomotheticism—that is, toward probabilism.

Two lines of development issue from classical behaviorism. One is the formalized, nomothetic behaviorism of Hull and his associates at Yale which I have already mentioned. The other and more radical departure is the "purposive" behaviorism of Edward C. Tolman at the University of California (*17*). It is best introduced by first referring to the fourth of the major crisis schools, frequently called American functionalism. Historically, this school precedes, parallels, and is about to outlive classical behaviorism. Under the influence of Darwin and other evolutionists, functionalism is characterized mainly by an emphasis on the readjustive value of behavior in coming to terms with the physical or social environment. An early representative was Thorndike with his problem-solving experiments with animals and his "trial-and-error" principle. In contrast to frequency of repetition, which played such a large part in the nonsense syllable experiments of Ebbinghaus and the "conditioning" experiments of the Russian physiologist, Pavlov, Thorndike stressed the importance of success and reward in learning ("law of effect").

Tolman's purposive behaviorism combines the constructive elements of classical behaviorism with those of functionalism and of Gestalt psychology in a program of animal and human experimentation and theory that is both "objective" and "molar." Its redefinition of purpose is operational, stressing the reaching of common end-stages from a set of differing initial or mediating stages. Thus it is not less or more "teleological" in the objectionable, vitalistic sense than Wiener's and McCulloch's cybernetics.

Convergence with a further type of influence from systematic theorizing in the physical sciences may develop if von Bertalanffy's attempted reduction of biological "equifinality," and thus of purposive behavior, to Prigogine's new open-system thermodynamics (*18*) should obtain the approval of physicists and biologists. It is one of the intrinsic limitations of high-complexity disciplines such as psychology that final judgment of reductive theory of this kind must remain outside his province. Since the verdict of history is not yet in, I have bracketed these developments in Table 4.

Claude Bernard's and Cannon's ideas on homeostasis have received increased attention in functional psychology, and Ashby has used them in his *Design for a Brain* (*19*). Ashby's book is part of a current vogue in brain models which has developed out of the earlier examples I have mentioned in connection with physiological psychology and which has recently received a further impetus from the interest in the engineering problems of complex calculating machines. With a more distinctly nomothetic slant, Rashevsky and his associates at the University of Chicago (*20*) have produced biophysical mathematical models that promise to be fruitful up to the level of social psychology and perhaps even in history.

Much of the work just mentioned, notably that on homeostasis and mathematical biophysics, proceeds under the assumption of a widespread isomorphy among outwardly diverse types of processes. Von Bertalanffy has therefore suggested the concerted development of a "general systems theory" (*21*). Such a theory could achieve a great deal toward the unification of the sciences under the auspices of the nomothetic-reductive approach that is so closely associated with the history of the natural sciences.

Diversification of Psychology as a Probabilistic Science

At this point we must pause and take a look at the foundations of our discipline. There is nothing in the development of science that will inspire paralyzing awe and induce adolescent dependence as much as will a headway in modes of conceptualization such as the natural sciences have been found to possess in relation to psychology. We must therefore be doubly on our guard against the intrusion of policies that are alien to our basic problems. In particular, we must ask ourselves whether the following of the nomothetic lead is an unmixed blessing for psychology.

Let us fall back on the thema of psychology as we have tried to develop it in analyzing the differences between physiology and physiological psychology. We have conjectured that the emphasis on wide-spanning functional correlations

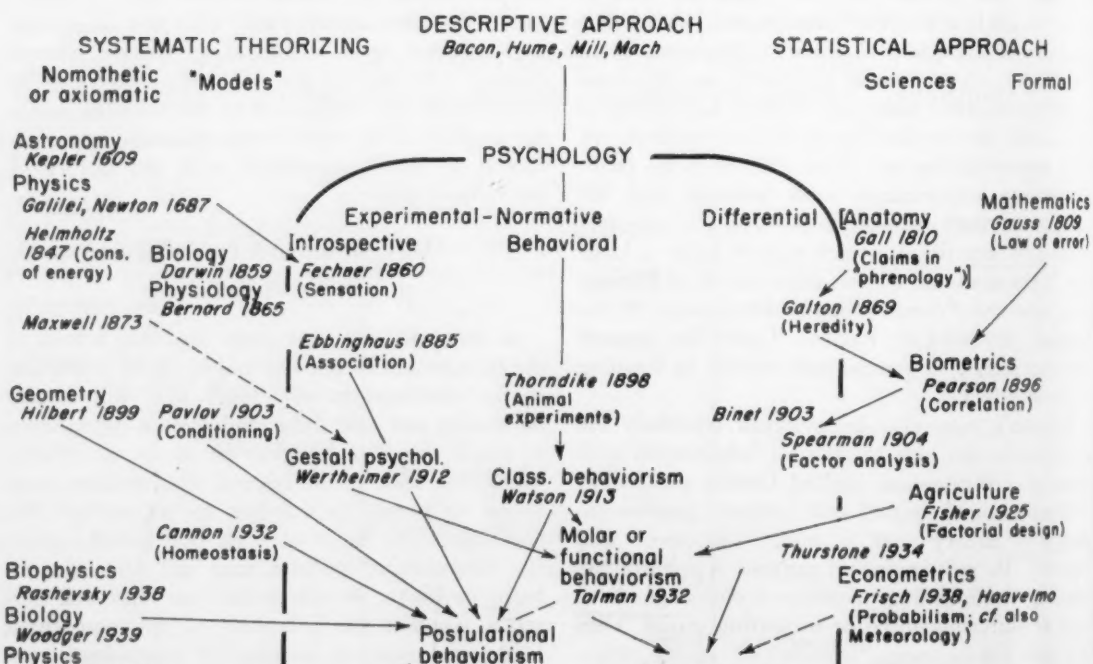
at the expense of attention to the intervening technologic detail is one of the major characteristics that distinguishes psychology from its predecessors (diagram *C* of Table 1). Tolman's molar behaviorism, and parallel developments in the study of perceptual "thing-constancy," can be shown to fall in essentially the same pattern, the fact notwithstanding that the focus in the central region remains hypothetical (*6, 22*).

On further analysis, we note that the functional arcs that span toward, and gain their feedback from, the remote, "distal" environment—and these are the really important arcs—become entangled with the exigencies and risks inherent in the environment. So long as the organism does not develop, or fails in a given context to utilize completely, the powers of a full-fledged physicist observer and analyst, his environment remains for all practical purposes a semierratic medium; it is no more than partially controlled and no more than probabilistically predictable. The functional approach in psychology must take cognizance of this basic limitation of the adjustive apparatus; it must link behavior and environment statistically in bivariate or multivariate correlation rather than with the predominant emphasis on strict law which we have inherited from physics.

It is perhaps more accurate to say that the nomothetic ideology was influenced by a somewhat naive and outdated high-school-type, thematic cliché of physics which some of us have tacitly carried with us in the process of developing psychology into a science. As need not be reiterated here, physics itself has become statistical in the meantime. Yet we must not fall for easy analogy; we must make it clear that the satistical underpinning which experimental physics has received in statistical mechanics and quantum theory, being of a microscopic character, has little to do with the probabilism of functional psychology. Since the individual case or instance does not lose its identity in psychology, our form of probabilism is macroscopic rather than microscopic and thus of much greater consequence in the actual execution of our discipline.

The use of bivariate correlation statistics is most

Table 4. Scientific background and cross-disciplinary relations of psychology with special emphasis on general systematic isomorphisms and on methodology.



obviously inevitable in the study of individual differences. In reality, the two have developed in close contact with one another; I have therefore grouped them together on the right side of Table 4 rather than placing the statistical approach where it belongs—that is, halfway between the nomothetic and the purely fact-descriptive approach.

As long as correlation statistics was not available, problems of differential psychology were treated with an air of absoluteness that reminds one of the nomothetic approach. One of the most grotesque products of this would-be type of correlational study was the so-called "phrenology" of the anatomist, Gall, who tried to link mental faculties to the shape of the skull on the basis of the most casuistic evidence.

Psychology has from its beginnings been intertwined with statistics and its active development in a variety of ways. Classical psychophysics is closely linked to the study of unidimensional error distribution and thus to Laplace and Gauss, notably in such American psychophysicists as James McKeen Cattell. Bivariate correlation statistics was first introduced by Galton and Pearson and may even be considered to be a direct joint product of biometrics and psychometrics. The factor analysis of Spearman, of Thurstone, and of others is even more a distinctly differential-psychological development. Because of the partly mathematical character of most of these developments, I have placed them somewhere between differential psychology and formal statistics in Table 4. For the first time in this analysis, we witness a give-and-take between psychology and other disciplines (as brought out by the zig-zag course of arrows in the table); we may take this as an added omen of the inherently statistical character of psychology as a whole, briefly deduced in a preceding paragraph.

While statistical correlation and factoring, including the attendant representative sampling of individuals from a population, have traditionally been recognized as necessary in the area of individual differences as they occur in intelligence or personality testing, the possibility of studying the broader functional organism-environment relationships by these statistical methods has long been ignored. In certain essential respects, this even holds true for R. A. Fisher's factorial design and analysis of variance (*23*). The variables and their levels of strength are still arbitrarily selected rather than naturally sampled; hence the results are subject to severe limitations of generalizability. By allowing multivariate analysis, however, Fisher's methods are definitely superior to the older univariate designs, especially in the study of problems of higher complexity. It is therefore not astonishing that their first transfer from their original domain, agriculture, to psychology (*24*) fell within the framework of Tolman's molar-functional study of behavior; their use has since rapidly expanded over wide areas of experimental psychology.

To obviate the intrinsic shortcomings of the artificial, "systematic" designs of which factorial design is but an elaborate case, I have advocated that in psychological research not only individuals be representatively sampled from well-defined "populations" but also stimulus situations from well-defined natural-cultural "ecologies" (*25*); only by such "representative design" of experiments can the ecological generalizability of functional regularities of behavior and adaptation be ascertained. Representative sampling of situations from the ecology allows us to take cognizance of the occasional major failures that result from the fallibility of perceptual cues or behavioral means while at the same time fully recognizing the favorable cases also. Generalization of the achieved degree of success to the ecology as a whole becomes possible with the use of the routine technical criteria for sampling statistics hitherto confined to differential psychology. Since representative design has so far been used only in a limited set of contexts, it has been bracketed in Table 4.

As was pointed out by Hammond (*26*), certain parallels can be drawn between the situation created in psychology by representative design and the situation created in physics by relativity theory. In both cases, an earlier overgeneralization of classical results obtained within a limited type of universe is corrected. As has been pointed out by Hammond in a different context, factorial design is geared to the manufacturing type of problem situation that prevails in agriculture, while the organically developed natural-cultural range of generalization to which psychology must aspire demands other methods. As in the example from antiquity which was discussed in the second section, we are faced with a warning against the uncritical transplantation of content-alien themas or instrumental scaffoldings, at least as long as we wish to prize and uphold our indigenous thematic identity above all else.

Perhaps the first to see that psychology was not a "fundamental" discipline in the sense of the standard natural sciences was the Columbia psychologist, Woodworth (*27*). Representative design and the resultant probabilistic functionalism are nothing but the consistent projection of such a belief onto the plane of methodology and explicit

theorizing. Since the statistical macroprobabilism of representative design would move psychology away from physics and other fundamental natural sciences with their nomothetic thema, considerable resistances must be expected along the way (*28*).

Our thematic submissiveness to the physical sciences may be more readily overcome if we can show that the proposed reorientation would bring psychology closer to other, perhaps less glamorous but no less urgent or real, natural and social sciences in which macroprobabilism has long been recognized as a legitimate attitude. Prominent among these admittedly and recognizably statistical disciplines are meteorology (*29*) and economics insofar as they use autocorrelation and intercorrelation, as theoretically stressed especially by Wiener (*30*), for probability prediction. Economics deserves special attention in view of the reinforcement which the statistical conception has received in the Norwegian school of econometrics, notably by Haavelmo (*31*). Within still another discipline, communication theory—as cast into mathematical form by Claude Shannon, with psychologically cogent commentary by Warren Weaver (*32*)—the study of message transmission through semierratic external media comes very close to the psychological problem of the anticipation of, and adjustment to, a distant world. Communication and psychological functioning also have in common the use of redundancy as a means of overcoming the low predictive probability of single signals of these instrumentalities (*6*, chapt. 2, and sect. 23). In probabilistic functionalism, this low predictive probability finds its counterpart in the limited "ecological validity" of cues or means, and the place of redundancy is taken by the intersubstitutability or "vicarious functioning."

Conclusion

The growing strength of behaviorism has long assured that the core of a procedural physicalism and thus of the essential operational aspects of the unity of science are rapidly becoming a matter of course in psychology. The time has come when unity of science is best served by stressing the thematic differentiation among the sciences within the over-all unity. In carrying this diversificaton to its logical conclusion, psychology emerges as a macrostatistical discipline, thus acquiring not only distinct thematic identity but also internal methodological unity. The acceptance of this probabilistic functionalism and of the attendant representative design of research may be facilitated, both inside and outside psychology, by a comparative methodology involving sciences of all shadings. The best way to emancipate psychology from the suggestive power of those nomothetic-reductionist natural sciences in the shadow of which it began its development is to demonstrate its structural affinity with disciplines already recognized as statistical in character.

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Our Neighbor Mars

The photograph on our cover this month shows the unusual atmospheric belts of Mars. This picture was made in the summer of 1954 by E. C. Slipher when he was leader of the National Geographic Society—Lowell Observatory Mars Expedition to Bloemfontein, South Africa. Blue-light plates picked up the bands for the first time on only a few nights during the 1954 opposition. Here the bands parallel the equator. A blue-white canopy, probably of fine ice crystals, mantles the north pole (at bottom of globe). The south polar cap, of thin ice or hoarfrost, gleams whiter and wider in the winter hemisphere (at top).

During the 1954 expedition a new dark area was clearly seen on Mars. This marking had a blue-green tint and covered an irregular area of approximately 200,000 square miles—a little less than the size of Texas. Its center lay in Martian longitude 235 degrees and latitude 20 degrees north, near where the great Thoth Canal joins the Aquae Calidae. According to Slipher, this wholly unexpected modification represents the greatest change observed in the geography of the planet since its surface was first mapped 125 years ago. For the 1954 observations, the National Geographic Society–Lowell Observatory party made use of the 27-inch refracting telescope at the University of Michigan's Lamont-Hussey Observatory in Bloemfontein.

On 6–7 September, Mars will be only 35,300,000 miles from the earth, and Slipher is again in South Africa continuing his observations. An article on "A new interpretation of the surface of Mars," by Dean B. McLaughlin, of the Observatory of the University of Michigan, is scheduled for publication in the October issue of *The Scientific Monthly*. In this article, McLaughlin summarizes his volcanic-aeolian hypothesis of the surface markings and brings together in one place material that has been published in widely scattered fragments.

BOOK REVIEWS

A Short History of Medicine. Erwin H. Ackerknecht. Ronald Press, New York, 1955. xviii + 258 pp. Plates. $4.50.

This is an excellent account of the history of medicine and I can recommend this book without reservation to everyone working in any area of human biology. *A Short History of Medicine* is written by a person who has read very widely on historical medicine. Moreover, this book is written with a style and comprehension that bring new life to an important and much neglected area of medicine. Students and practitioners of medicine will obtain a greater appreciation, understanding, and knowledge of modern medicine by reading this stimulating account of the history of medicine.

This book was written in a manner suitable for people who may want to familiarize themselves with this field without wading through numerous and highly detailed articles and books. The volume will be of real service not only to the medical student, the busy physician, and others associated directly or indirectly with the medical profession, but also to laymen who are becoming increasingly aware of the biological world as well.

The author has used his extensive background as a professor of the history of medicine at the University of Wisconsin Medical School to highlight effectively some of the most important features of historical medicine. He does this in an enlightening, interesting, and yet very brief manner in a total of 223 pages, exclusive of the appendix and index. One must have a thorough speaking acquaintance with numerous facts and facets of medical history to accomplish successfully such a satisfying literary exposition as one finds in *A Short History of Medicine*. This book is exactly what its title indicates—it is a short history of medicine. It will effectively introduce the subject to those who may wish to acquire more extensive knowledge of medical history. It will thus help them gravitate to the more detailed volumes written by the author's teacher, H. E. Sigerist, and by the associates of the author and Sigerist for an extension of knowledge along this animated backbone of our medical history.

Since so much has been written on the history of medicine, one will naturally inquire concerning the need and publication of the present volume. The need is both apparent and real. Our students should have these facts at their command so that they might better understand the evolution of medical thought, discovery, practice, and experimentation. I think it is a fair statement to say that up to this time most of our students have been afraid to delve into the history of medicine because most of it has been included in massive volumes. This compact volume will help renew interest in the subject. The orientation for a detailed approach to the history of medicine is provided in this book.

The author lists the following reasons for interest in medical history: (i) to gain a better understanding of history in general: through its history, the medical behavior of a period can be regarded as a kind of a projective test of the total culture of that period; (ii) to realize the desire to understand medicine itself and to grasp its techniques, its organization, and its underlying ideas; (iii) to understand the complexities of medicine and how it became highly specialized; and (iv) to obtain a working guide to deal more effectively with patients as a result of pursuing a vast accumulation of medical facts pertaining to our present level of medical comprehension. Ackerknecht clearly shows how these factors are achieved by citation of a superabundance of examples taken from the stimulating chapters of medical backgrounds. This interesting and engaging book will take the reader beyond these points, especially since it stimulates thought—an aim noteworthy in itself.

On the other hand, one might say that he is a competent doctor without a knowledge of medical history, and to this our author quickly shows that an acquaintance with medical history can make one a better doctor. I am in wholehearted agreement with Ackerknecht that ". . . it is no accident that so many of the great doctors of the last hundred years, whether the name be Osler, Halsted, Welch, Cushing, Andral, Virchow, Wunderlich, Claude Bernard, Charcot, Pasteur, Behring or Sauerbruch, have had a profound interest in medical history and have often made valuable contributions to the field." Furthermore, it is becoming readily apparent that medical history repeatedly shows the student how the truth of today becomes the error of tomorrow, instills a more independent and critical attitude in his work, and better equips one to assimilate new truths.

This book is divided into 20 chapters ranging from paleopathology and paleomedicine, primitive medicine, medicine of ancient civilizations, ancient India and China, Greek medicine—physicians, priests, and philosophers, Hippocratic medicine, Alexandria, and Rome to medieval medicine, to Renaissance medicine, and to modern-day medicine, by way of a critical evaluation of medicine

during the 17th, 18th, 19th and the first half of the 20th centuries. The author's literary exposition is graced with the inclusion of 28 very significant illustrations.

Of particular interest to me was the chapter on "Medicine of ancient civilizations." Some of the earliest medical documents, the *Kahun Papyrus* (about 2000 B.C.), the *Edwin Smith Papyrus* (about 1600 B.C.) and the *Ebers Papyrus* (about 1550 B.C.), are discussed in great detail. In reading this chapter, one obtains a dual picture of the medicine of this era: clinical acumen coupled with Egyptian supernaturalism. The anatomy as viewed in the *Ebers Papyrus* is definitely of a speculative nature. It is truly incredible that a nation of embalmers (Egyptians) did not acquire a knowledge of anatomy beyond their concept of the existence of heart, 44 (or 22) hypothetical vessels, and the life-giving role of the breath. The development of medicine in ancient Mesopotamia saw many differences from that of the Egyptians. The Mesopotamians used clay tablets for the recording of their medical knowledge, and these tablets have survived long enough to tell us about such things as physicians' seals, legal codes governing the physician and surgeon, and the culture of these people as reflected not only in medicine but in their religion as well, the latter occupying a major role in the former. One will also note that dreams had a special meaning during these early times. True, some of these facts may well have been explored in other treatises, but the present one is by far the most efficient treatment that this subject has received to the present time.

In summary, I thoroughly enjoyed reading this book, and the author and publisher are to be congratulated for their contribution to both the medical scientists and laymen alike. Herein lies a very interesting description of medical world thought that comprises our present-day comprehension of those growing pillars of medical information referred to by such medical geniuses as Virchow, Claude Bernard, Louis Pasteur, Koch, Lord Lister, Jean Martin Charcot, Bayliss and Starling, and Harvey Cushing. I recommend this book without reservation because it highlights (i) medical trial and error, (ii) medical philosophy, (iii) medical discovery, and (iv) the job ahead for every patient —and sometime in our lives we are patients. One cannot know too much about the history of human biology, and this book will help to provide an investigative pattern of thought concerning the concepts of our living bodies.

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Sir William Petty. Portrait of a genius. E. Strauss. Free Press, Glencoe, Ill., 1954. 260 pp. Illus. $5.

"One science only will genius fit,
So vast is art, so narrow human wit."

So run the lines of Alexander Pope's *Essay on Criticism*. But Sir William Petty was different. His genius was multiphasic. And the two fields that he became identified with by conspicuous achievement formed a rare combination: medicine and economics.

This book is an admirably organized profile of Sir William Petty, whose fabulous career stretched across the tumultuous course of 17th-century England and Ireland from 1623 into 1687. In an engaging but professional manner, Strauss presents the zigzag pattern of Petty's life and interests to us as he successively became a cabin boy at 14, hawker, medical student, professor of anatomy at Oxford, physician to the Army of Ireland, fortune builder, demographer, and distinguished economist. The book, however, is not a conventional "full length biography." Its main focus is on the character of Petty, the forces that molded and stimulated him, and his creative contribution to the growth of science and modern social science. In this objective, the author has had considerable success.

Strauss capsules the main subdivisions of his study into three titles: "Endeavor," "Frustration," and "Achievement." The first centers on Petty's early life, including his work as a professor of anatomy at Oxford and the events leading to his acquisition of immense land possessions in the midst of the unhinged political conditions of Ireland under the Commonwealth.

Part II tells us of the disappointing and time-consuming efforts to defend these possessions through endless litigation—struggles that continued to the close of Petty's life. And here we also discover the complex facets of personality. Petty, in common with most geniuses, was a man not easily winded. But his prodigious energy and gifted mind never quite compensated for the lack of traits that prevented him from reaching the high political and administrative office he sought. Had he won this recognition, which his genius definitely commended, he would have had the golden opportunity of dipping deep into the vast bin of ideas he held about economic policy, shipbuilding, social care, religious toleration, and other subjects. But the opportunity never came. Sir William apparently seems to have been too tactless, too impatient with the ways of politics, and very likely too argumentative.

Petty's foremost and lasting contribution was his work in the formulation of the Labor Theory of

Value and in its creative use for the solution of economic problems. This work, along with many other durable landmarks, the author appropriately leaves to the third and last section—"Achievement." The paramountcy of Petty's contributions obviously lies in his application of quantitative data to social and economic facts. Much of what we call survey analysis today was anticipated by Petty—a man who was the true social scientist in the advancement of techniques. But he was also more versatile. He was capable of not just thoughts, but thought, and he was remarkably farsighted in sensing the shape of things to come in our own 20th century. For a meaningful look at this genius at work astride a complex period of history, we are much indebted to Strauss.

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Hastening Childbirth without Pain. P. A. Beloshapko and A. M. Foi. State Publishing House of Medical Literature, Moscow, U.S.S.R., 1954. 173 pp. Illus. (In Russian)

In 1851, shortly after Morton and Simpson independently demonstrated the feasibility of inhalation anesthesia, V. M. Shklyarskii, a Russian obstetrician, reported the successful use of intermittent chloroform and ether anesthesia in childbirth. In 1881, S. K. Klikovich, a resident in the therapeutic clinic of S. P. Botkin, the father of Russian scientific medicine, summarized his experience with nitrous oxide as an anesthetic agent in childbirth.

Despite this early start, the utilization of any form of analgesia in Russian obstetrics remained on a rather limited scale. The first evidence of extensive investigations in this field appeared with the publication by A. P. Nikolaev in 1924 of his results of utilizing pharmacologic agents and methods of suggestion and hypnosis in achieving painless childbirth. A great number of publications in this field appeared thereafter, many of them dealing with basic as well as clinical investigations of problems of hastening childbirth and rendering it painless. During the years the emphasis of Russian obstetrics has gradually shifted from pharmacotherapeutics to that of psychoprophylactic analgesia. This point of view is essentially similar to that expressed by Grantly Dick Read in his book *Childbirth without Fear.*

The present publication by two outstanding Soviet obstetricians summarizes the experiences of Russian physicians in this field. The book, as one of a series of monographs designated "The library of the practical physician," does contain a great deal of practical detailed instructional material. These "tricks of the trade" include prescriptions of anesthetics and of other drugs when indicated, methods of psychological and physical preparation of pregnant women (including the types of exercises), techniques for evaluating the effectiveness of the methods used by physicians, and so forth. However, the compendium goes far beyond the framework of a technical handbook by including substantial discussions of basic physiological mechanisms and by pointing out some of the unsolved problems.

The method of choice of Soviet obstetricians appears to be that of prophylactic analgesia. The guiding philosophy for this preference is approximately as follows. Since childbirth is a natural physiological phenomenon, there is no reason to assume that pain is either essential or inevitable. Many women, according to the authors, experience little if any pain, although they may feel the uterine contractions during parturition. Birth pains in many women are chiefly a result of fear, overexcitation, and consequent exhaustion of the cerebral cortex. The authors emphasize the great importance in man of symbols as stimuli that can influence via the cerebral cortex various visceral and somatic functions. In Pavlovian terminology, this refers to the important role played in human beings by secondary signal system conditional reflexes in response to words or memories as stimuli.

For these reasons, the major attention of Soviet obstetricians is directed toward the proper psychological and physical preparation of pregnant women for the act of parturition. Russian obstetricians pay at least as much attention to the emotional and mental condition of a pregnant woman as to her physical state. The mother-to-be is taught that childbirth pain is not inevitable. Attempts are made to develop a happy and positive forward-looking attitude toward the great event. The woman is instructed in methods of actively participating in the act of childbirth, the correct and properly-timed utilization of uterine contractions, relaxations between contractions, breathing exercises, the use of analgesic trigger areas, and so forth. The attention of the woman is thus drawn away from fear of pain and is concentrated on her role as an active participant together with the medical personnel in efforts to achieve a natural painless childbirth. Group instruction, based on classes selected according to the educational background of the women, made possible the application of the method of psychoprophylactic analgesia on a mass scale. According to the authors, this method is widely used in all rural as well as urban areas of the U.S.S.R.

It is interesting that the book contains no reference to the pioneering work of Grantly Dick Read, nor does Read refer in his book to the experiences of the Soviet obstetricians, although Read began the development of his method at about the same time as the Russians. This mutual ignorance is most probably not a result of studied neglect or disrespect but is more likely a demonstration of the lack of communication between Soviet and Western scientists and physicians. It is tempting to make comparisons between the observations and conclusions independently arrived at by Read and by the Soviet physicians.

In each case, the starting point that prompted the reevaluation of the problem of birth pains was represented by the observation that some women do not apparently experience any appreciable pain during labor. Read describes the case that started his thinking along these lines. A poor woman in Whitechapel kindly but firmly refused the chloroform mask during labor. After an uneventful and calm delivery Read inquired why she refused the mask. Her answer was simple: "It didn't hurt. It wasn't meant to, was it, doctor?"

Read observed many other deliveries that appeared to be essentially painless. Yet many others obviously suffered agonizing pains. Why? Careful, keen observations suggested to Read and to the Soviet obstetricians that, in most cases, painful labor was associated with fear, tenseness, and a host of other negative emotions, the latter often accentuated by the obstetrician who insists on using anesthetics as a matter of course and who treats the mother-to-be in the same matter-of-fact, detached manner as one treats a test tube in the laboratory. Eventually it became apparent to those who observed carefully that only a minority of deliveries (usually those with complications) need have pain requiring anesthetic agents. Many women with proper preparation can deliver with relatively little or no pain.

Like Read, the Soviet obstetricians imply that perhaps many women may have been forced into the acceptance of anesthesia by an obstetrician displaying his authority as a licensed physician rather than using his wisdom as a man of science and a keen observer. In many a case, by the time the woman is in labor, large doses of sedatives and anesthetics may actually be needed as a result largely of the attitude displayed by the physician throughout the entire period of pregnancy. The poor woman expected pain and she got what she anticipated.

This exclusive and systematic reliance on miracle drugs, rather than on prevention of undesirable pathologic developments is simply one aspect of our emphasis on finding cures or methods of obtaining symptomatic relief rather than on the prevention of disease.

Could it be that we have created too much of an iron curtain between science and philosophy? Could it be that too many scientists have become habituated to the exploration of how to do things without asking questions about why and whither? Could it be that we need to develop more extensive communication channels between all the branches of human knowledge, including the social sciences and philosophy?

I remember that my very first queries of "why" in freshman laboratory science courses were invariably answered by authoritative statements by the instructors, pronounced with the air of an obvious axiom, to the effect that science never asks "why," only "how." Perhaps too many of us succeeded too well in acquiring the habit of not asking "why" and have managed to execute well and elegantly many a precise but foolish procedure.

These are the thoughts that are provoked by reading the experiences of the Soviet obstetricians or of Read. This book differs from that of Read by the inclusion of a discussion of basic physiological investigations in the prevention of pain as well as in problems of hastening labor. This monograph also has a better balance than Read's *Childbirth without Fear* because of the inclusion of other methods of producing analgesia in cases where the psychoprophylactic approach appears to be inadequate.

Among the other methods used are (i) suggestion with and without hypnosis; (ii) pressing on certain analgesic trigger areas; (iii) counterirritants applied to the abdominal region; (iv) local anesthetics; (v) intermittent anesthesia with ether or nitrous oxide (80 percent NO_2 and 20 percent O_2); and (vi) a selected group of systemic analgesics consisting chiefly of certain antipyretic analgesics, opiate derivatives and some synthetic narcotics, and combinations of some of these with magnesium sulfate. The systemic analgesics were selected on the basis of the least depressant effects on the respiratory center of the newborn and the least harmful effects to the mother.

A critical résumé of the experiences of Soviet obstetricians with the analgesics and with oxytocic drugs is given by the authors. The only form of local anesthesia advocated (during the second stage of labor only) is that applied to the pudendal nerve for which purpose they use chiefly novocaine or lupocaine (an ether of *p*-aminobenzoic acid and the alkaloid lupinin recently synthesized in the U.S.S.R.).

The use of counterirritants as a means of decreasing childbirth pains was reported by a Russian

obstetrician, P. I. Arkhangels'kii as early as 1897. He applied a mixture of ether and olive oil to the abdominal region. Soviet obstetricians have made extensive use of this method, utilizing a number of different counterirritants.

The authors explain the analgesic effects produced by counterirritants on the basis of I. P. Pavlov's principle of negative induction. Intense stimuli from the skin create in the cerebral cortex a new locus of excitation. The latter, according to Pavlov, is surrounded by an area of inhibition, thus preventing the perception of pain stimuli. The counterirritants employed by the Soviet obstetricians produced analgesia for periods of about 1 hour.

The psychoprophylactic method remains as the basic means for the management of natural childbirth without pain. The other methods are considered as adjuncts whenever complications arise or when the woman was not properly prepared.

The effectiveness of the psychoprophylactic method when used alone varied in different institutions. The obstetric clinic of the First Pavlov Medical Institute of Leningrad reported successful analgesia in about 75 percent of their cases. A. P. Nikolaev *et al.*, working at the Institute of Obstetrics and Gynecology of the U.S.S.R. Academy of Medical Sciences, reported complete analgesia in 52.3 percent of their cases. Thirty-four percent experienced mild pain during some stage of labor; 11 percent obtained partial analgesia, while in 2.7 percent the method was ineffective. I. Z. Vel'vovskii, one of the Soviet pioneers in natural childbirth, reported successful analgesia in 90 percent of his cases.

Subsequent observations, according to the authors of this book, indicate that the percentage of successful analgesia may not be as high as those reported here. The divergent reports are ascribed to unsatisfactory methods of evaluating analgesia and to differences in the effectiveness of organization of psychoprophylaxis in different institutions.

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Man and Energy. A. R. Ubbelohde. George Braziller, New York, 1st American ed., 1955. 247 pp. Illus. $5.

This book germinated out of a series of talks delivered by A. R. Ubbelohde on the British Broadcasting Company Third Programme. The author explains the theme of the book in the preface: "The bulk of scientific progress is so great that the whole tends to become lost in mere encyclopedism. However, one central theme which ties much effort together is the development of the various relationships between man and physical energy. This book surveys the astonishing growth of human power over energy from the prehistoric domestication of slaves and draught animals to the modern control of the atomic bomb." (Perhaps the author really meant "control of atomic energy.")

Another quotation (p. 16) will help establish the author's point of view: "It is a widespread mistake to describe our present age as the 'Machine Age.' Admittedly machines of great power and helpfulness are active daily in town and country. . . . Yet what really marks out our civilization from all previous experience in the history of mankind is the nature of the power used to drive machines. It is common knowledge that in those parts of the world which are most advanced technologically the overwhelming proportion of machines are driven by inanimate energy."

The author develops his ideas in an interesting and effective way. He focuses attention first on sources of energy themselves and their increased exploitation by man and then on the growth of man's understanding of energy. Mechanisms for energy utilization, machines, gadgets, and so forth, are considered only to a minimum extent.

The first part of the book, entitled "Growth of power," traces the growth of man's control over energy. This part contains both a brief historical account and a collection of illustrative tables. The author is very much interested in the influence of advance in technology on society and spends considerable time on this point. He introduces the concept of an ideal society that has its work largely performed by inanimate energy slaves, and he calls his society *Tektopia* (from technology and utopia.)

The second part of the book, entitled "Growth of knowledge," traces the growth of man's understanding of the control of energy. An account is given of thermodynamics and the interpretation of thermodynamics in terms of molecular behavior (statistical thermodynamics). Here again the author is interested in the broader aspects of his subject and discusses the influence of the ideas developed in thermodynamics in other fields, such as the understanding of life-processes and cognition.

There are many interesting facts and stimulating ideas in this brief book. It should be read with profit by scientist and nonscientist alike.

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The Hopi Indians. Their history and their culture. Harry C. James. Caxton, Caldwell, Idaho, 1956. 236 pp. Illus. + plates. $5.50.

The Hopi, westernmost of the Pueblo Indians, occupy villages built on the fingerlike projections of Black Mesa in northeastern Arizona. They are farmers and herders today, and the groups from which they were formed have been farmers in their arid country from at least the sixth century. Much of the old culture has been conserved, a circumstance that has made the Hopi of high interest both to ethnologists and tourists.

This book is not a contribution to the ethnology of the Hopi but a pleasant introduction, a well-illustrated and designed guide, for the general reader. It combines documentary and legendary history with anecdotal sketches of village life, an appreciation of Hopi crafts, and a brief account of the aspect of Pueblo life that draws swarms of visitors to the mesas—the kachina and social dances. Six Hopi tales are included in a final chapter.

It is the work of a professional writer (some of the material has appeared in *Desert Magazine* and the *Christian Science Monitor*) who is a long-time friend, a keen and kindly observer of the people, and a collector of their handicrafts. The book is commended by Frederick Webb Hodge, who read the manuscript critically and contributed a preface.

We would guess that if the ethnographers of the Hopi with whom H. C. James shows familiarity were to present their own popularizations, they might supply a more subtle accuracy of emphasis and balance with respect to cosmology, they would be loath to infer complexity and mystery where it is absent in social organization, and they would avoid even innocent lapses into biological interpretations of cultural behavior. But the present kind of book, and this particular book, is justified and welcome.

It is evident that Hopi culture is a vigorous, resilient and viable one, which, despite some inherent social weaknesses, particularly in its political aspect, has provided mechanisms for individual and group survival through times of trial and crisis —a process spoken of in our own folk culture as the triumph of the human spirit.

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Books Reviewed in SCIENCE

6 July

Physics of Fully Ionized Gases, L. Spitzer, Jr. (Interscience). Reviewed by L. L. Loeb.

Advances in Genetics, vol. VII, M. Demerec, Ed. (Academic Press). Reviewed by E. L. Green.

Resonance in Organic Chemistry, G. W. Wheland (Wiley; Chapman & Hall). Reviewed by R. H. Eastman.

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